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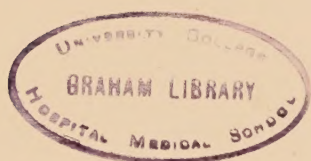
BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

VOLUME 1

BIOGRAPHICAL MEMOIRS OF FELLOWS OF THE ROYAL SOCIETY

1955

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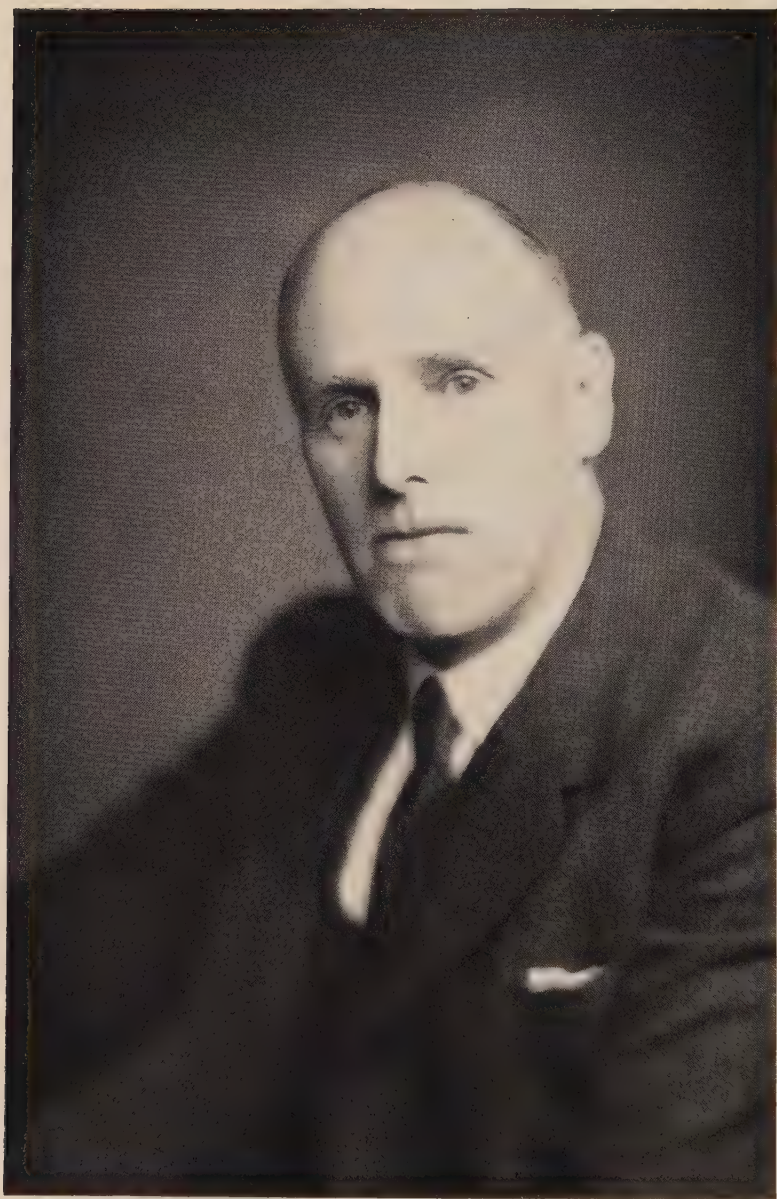
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A photograph of the subject is printed with each notice.



Wallace A. Akins



WALLACE ALAN AKERS

1888-1954

IN writing about SIR WALLACE AKERS it is not easy, within limited compass, to do justice to so many-sided a personality.

By profession he was a chemist, educated at Aldenham School and at Christ Church, Oxford. His speciality was physical chemistry.

Upon leaving the University he joined the staff of Brunner Mond & Company at Winnington, Cheshire, where a laboratory had been established, one of the first of its kind in this country, to pursue fundamental research. While there he produced various reports, including one in which as a former colleague has testified 'he showed an unrivalled capacity for elucidating crucial data from a welter of technical figures of varying accuracy and for applying to these data the principles of thermodynamics with clarity and effect'.

In 1924 he joined the Borneo Company and proceeded as General Manager to the Far East, where he remained until, in 1928, he returned to take up a position as technical manager in the recently formed Imperial Chemical Industries, in which his old firm of Brunner Mond & Company had been merged. He joined the Board of the Company in January 1941.

In the course of that year the war-time Coalition Government decided to set up under the general control of the present writer, then Lord President of the Council, a special organization to deal with the whole problem of atomic energy in its various aspects, including, of course, research and development. After the most careful consideration the choice of a Director fell on Mr Wallace Akers, as he then was, and his services were accordingly lent by his Company to the Government for that purpose. Experience showed clearly that no better selection could have been made. His wide knowledge, unbounded energy, even temper and absolute integrity fitted him admirably for a task which called for ingenuity, tact and organizing ability of a high order. He had not only to co-ordinate the activities of scientists of the greatest eminence here and abroad, but also to conduct negotiations of great delicacy in both the U.S.A. and Canada. His services were recognized by the award of a C.B.E. in 1944 and a Knighthood in 1946. In the latter year he reverted to his position on the Board of Imperial Chemical Industries and again took charge of research until in April 1953 he retired under the age limit.

His academic distinctions included an Honorary D.Sc. of Durham as well as the honorary degree of D.C.L. of Oxford University. There can be no doubt, however, that the distinction he treasured most was his election in

1953 to the Fellowship of the Royal Society. His abounding energy did not permit him to relax in his retirement for he continued to serve as a member of the Advisory Council of the Department of Scientific and Industrial Research, and in 1953 he rendered most valuable service on the Committee which drew up the plan for the future organization of work on nuclear energy which is now in operation under the authority of legislation promoted by the Government.

The foregoing account would, however, be entirely misleading if it concluded without reference to an altogether different side of Sir Wallace Akers's activities. From an early age he was an accomplished pianist, and his interest in the arts led to his appointment first as a member of the Scientific Advisory Committee of the National Gallery and later to the Board of Trustees. It was to these and similar activities that he devoted most of his energies after his retirement.

WAVERLEY

Addendum

SIR WALLACE AKERS AND IMPERIAL CHEMICAL INDUSTRIES

Inside the I.C.I. organization the years in the early '30's may be regarded as those when Akers's reputation as an outstanding man was established. It was during this period he gained much experience as Chairman of the Billingham Division: to be followed in due time with his appointment as one of the Technical Managers. In this capacity he was recognized as the Technical Manager who had a special responsibility for research work. When in due time he was made a member of the Board of I.C.I., his special interest in research was specifically acknowledged.

After his work for the atomic energy organization under the Lord President of the Council had been completed, he returned to I.C.I. in 1946 and resumed his position as I.C.I. Director for research, a position which he held until his retirement in 1953. During this time he helped to establish an outlook and a tradition which will long be remembered. His belief in the pre-eminent importance of research and the research outlook to his Company and to the community was deep and sincere and it was not by accident that during his period of office the practice was generally adopted of making the head of the research department in each I.C.I. Division a member of the Division Board. Just as he himself was always ready to present the research viewpoint to his colleagues on the main Board of the Company, so by this step he ensured that research was adequately represented at all subordinate levels.

Akers was well aware of the fact that in industry the ultimate objective of all research must be a practical one expressible in terms of such things as

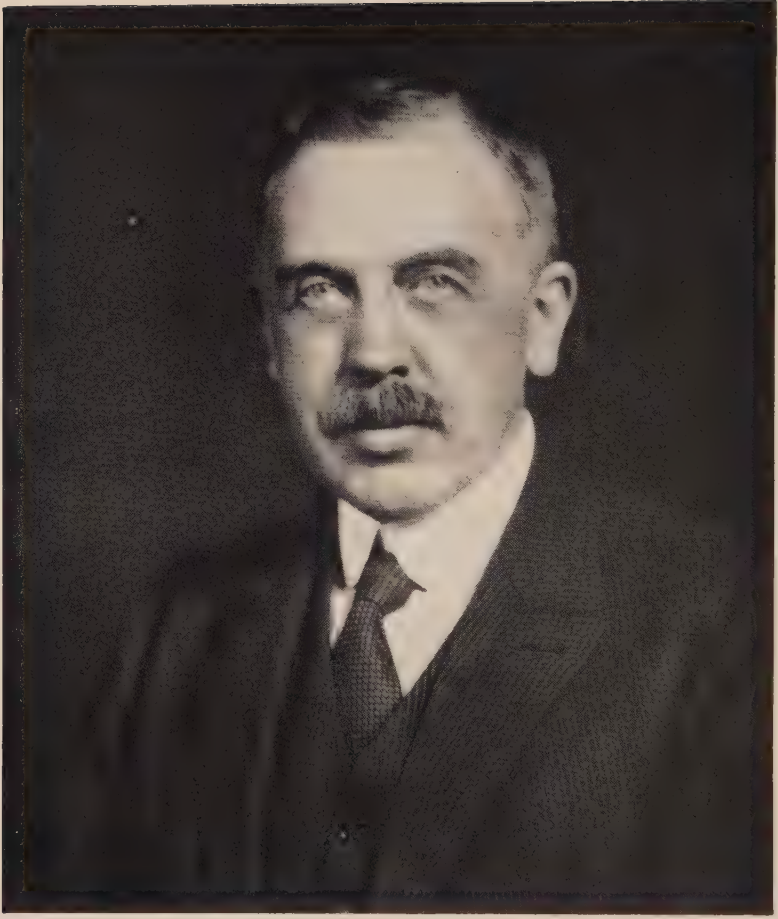
increased technical efficiency, decreased costs of production and expansion of business, but he also realized very clearly the importance of new fundamental discovery as a means to that end. Under his direction practical effect was given to the policy of encouraging fundamental research both within the Company and in the universities. University Research Fellowships were established in the award of which I.C.I. was most careful to be completely uninvolved and very substantial sums were made available year by year for the assistance of research in university laboratories. The decision to establish within I.C.I. a central research laboratory for long-distance investigations had been taken before the outbreak of war in 1939, but it fell to Akers to carry this plan into effect. The Butterwick Research Laboratories were set up in 1946 and the principles on which this establishment was founded and the form which it took were very largely his work. A research institution devoted wholly to the production of new basic knowledge and completely regardless of immediate practical objectives was and remains an almost unique feature of industrial life, and the recent decision to rename it the Akers Research Laboratories is a fitting tribute to his memory.

In his dealings with members of his department Akers believed in giving to each man the maximum of freedom and the maximum of responsibility. Some measure of administrative routine is unavoidable if any large organization is to avoid chaos, but he was never the man to insist on slavish uniformity for its own sake. He regarded his primary job as being to see that the most capable men were chosen for positions of responsibility under him, and that they were given as free a hand as possible to develop their talents in their own way. That he had in himself a grasp of scientific principles and a knowledge of scientific facts of no mean order must have been evident to any who had the privilege of hearing his well-known lecture on atomic energy. Nevertheless in his capacity of I.C.I. Research Director he never attempted to intervene officially in the detailed conduct of research, believing that this was best left to the man on the spot. He believed in encouragement rather than criticism, in inspiration rather than direction. He looked for the best in everyone and usually found it. He was known and welcomed in every I.C.I. research laboratory where his cheerful presence and modest suggestions catalyzed many successful projects for which he claimed no credit.

Akers possessed a prodigious memory, an acute brain and a speed of nervous reaction far above the average; these gave him the power to grasp the essentials of any situation in a flash, together with the equally important ability to discard the inessential. It was in consequence possible for him to keep very fully in touch with all that concerned him as a Director of the Company and Head of its research department and at the same time to devote a great deal of time and attention to outside interests. Although his name is not linked with any specific series of advancements in knowledge, yet he was instrumental in inspiring a great body of research work directed to both fundamental contributions and technological aims. Encouragement and inspiration—these were his methods, and the volume of his contribution

is not measured in the number of papers published under his name but by the widespread influence—organizational but above all personal—which bore fruit by the detailed work of other hands. In I.C.I. particularly, were to be found his outstanding characteristics: always the most approachable of men he was equally ready to talk or to listen; his friends are to be found in every walk of life.

ALEXANDER FLECK



H. S. Allen

HERBERT STANLEY ALLEN

1873-1954

HERBERT STANLEY ALLEN, Emeritus Professor of Natural Philosophy in the United College of St Salvator and St Leonard at St Andrews, died on 27 April 1954 at the Manse of Resolis, Balblair, in Ross-shire, where he had passed the last few months of his life with his daughter, Mrs Walker. He was born on 29 December 1873 at Bodmin in Cornwall and was the fifth son of the Rev. Richard Allen, who was Wesleyan minister there. He was very proud of his Cornish birth and that was no doubt the reason why his house in Hepburn Gardens, St Andrews, was named 'Lamorna'.

On his father's side his ancestry can be traced back to a certain Willielmus de Birkebeck who, as one of the adherents of the Earl of Lancaster in the reign of Edward II, was pardoned by consent of Parliament in the year 1318. A descendant of his, Thomas Birkbeck, was Governor of Carlisle and it is recorded that arms were granted to him, in 1515, for his brave defence against the Scots. Somewhat later, in 1553, the estate of Hornby, in the Parish of Brougham in Westmorland, was granted by the Earl of Cumberland to Edward Birkbeck and remained in the possession of the family till 1695. Allen descended from the Birkbecks through his great grandfather, John Allen of Caton, Lancaster, who married a certain Jane Birkbeck.

He received his early education (from 1886 to 1893) at the famous Kingswood School near Bath (Headmaster W. P. Workman, M.A., B.Sc.) and among his fellow pupils were T. M. Lowry, a well-known chemist and, in his later days, a professor in Cambridge; and the philosopher, A. E. Taylor, who was a few years Allen's senior and became, like him, a professor in St Andrews. While at Kingswood he became senior prefect and gained many scholarships and prizes. He actually took the first place in the London Matriculation Examination in 1892 and in the Cambridge Senior Local Examination a year later. He then entered Trinity College, Cambridge, and was elected to a Foundation Sizarship (£100 per annum) soon afterwards.

As an undergraduate he had rooms on H4 in Whewell's Court. During his first two years E. T. Whittaker (now Sir Edmund Whittaker) occupied rooms on H6 just above those of Allen, while in the latter's third year Whittaker was immediately below, on H1. In his early Cambridge days Allen studied mathematics and came under the influence of such great mathematicians as Forsyth and Whitehead. His private tutor was Mr R. R. Webb of St John's College. He graduated B.A. in 1896 with a first class in the Mathematical

Tripes, being placed 10th Wrangler. In 1897 he gained a first class in Part II of the Natural Sciences Tripes, physics being his principal subject, and was awarded an Exhibition and Walker Prize. He then worked for some time in the Cavendish Laboratory and in the Caius College Chemical Laboratory. From Cambridge he went to Aberystwyth to take up a post as Assistant Lecturer in Natural Philosophy and Astronomy; but very soon returned to Cambridge to undertake research work under the guidance of J. J. Thomson. This consisted in the experimental investigation of the motion of spheres through viscous fluids—a subject of great interest at that time because of the famous formula of Sir George Stokes and its application in the determination of the elementary charge of electricity.

Allen began the serious study of physical science at a time when the subject was passing through some of the most interesting phases in its development. Physical theory was still based on the mechanical principles laid down by Sir Isaac Newton; but the clouds which worried Kelvin had already appeared on the horizon. A formidable problem had arisen from the fact that the famous experiment of Michelson and Morley appeared to require that the luminiferous medium was at rest relatively to the earth, while the phenomenon of stellar aberration required that the earth was moving relatively to the luminiferous medium. In Allen's student days in Cambridge some progress was being made by G. FitzGerald and H. A. Lorentz towards the solution of this dilemma and it seems that W. Voigt was very near to it in the year in which the result of Michelson and Morley's experiment was published. Einstein's Special Theory of Relativity completely solved it in 1905. Lastly it may be mentioned that Max Planck published his first paper on the quantum theory in those days (14 December 1900).

In this year, 1900, Allen migrated to Renfrew to look after Lord Blythwood's laboratory. Here he met Miss Jessie MacTurk, daughter of the Rev. A. MacTurk, whom he married in 1907. At Renfrew he was principally occupied with research work on spectral photography, the Zeeman effect and radioactivity. The radioactivity of the water from the springs of Bath and Buxton was demonstrated in Lord Blythwood's laboratory in 1903. Allen had a share in perfecting the Blythwood dividing engine for ruling diffraction gratings with great accuracy to as many as 14400 lines to the inch. This is now at the National Physical Laboratory.

In 1905 Allen was appointed to a lectureship at King's College, London, and was promoted to Senior Lecturer about a year later. I became one of his (more junior) colleagues in 1906 when I obtained the post of assistant lecturer there. The physical laboratory at King's was then, and is still, known as the Wheatstone Laboratory after Sir Charles Wheatstone who was already a professor there when Victoria came to the throne. In Allen's early days at King's Harold Albert Wilson occupied the Wheatstone Chair of Physics, having succeeded W. G. Adams, a near relative I believe of the more famous J. C. Adams who, simultaneously with Le Verrier, predicted the existence and position of a new planet (Neptune). Charles Glover Barkla followed

H. A. Wilson when the latter left for America and Barkla was succeeded by Owen Willans Richardson (now Sir Owen Richardson) in 1913.

In 1909 Allen was awarded the D.Sc. degree of the University of London, his thesis being entitled 'Some problems connected with the discharge of electricity through gases'. From 1916 till 1921 he was Honorary Secretary (for papers) of the Physical Society, of which he was a Fellow for 49 years! While at King's College Allen and his wife lived at New Malden in Surrey where their two children, Frank and Margaret, were born. In 1913 he published an excellent work on *Photo-electricity* and in those days his experimental work was devoted mainly to the investigation of photo-electric fatigue. In 1916 he published, in collaboration with Dr H. Moore (later Professor of Glass Technology in the University of Sheffield), an elementary work on *Practical physics*, certainly one of the best of its kind that has ever been written. It has often been reprinted and is now, I think, in its 3rd edition.

In 1919 he joined his former chief, C. G. Barkla, as a lecturer in the University of Edinburgh and was soon promoted to reader. He became a Fellow of the Royal Society of Edinburgh in 1920 and later served on its Council. The MacDougall Brisbane medal was awarded to him by the Society for the period 1922-1924.

In 1923 he succeeded Professor Butler as Professor of Natural Philosophy in the United College of St Salvator and St Leonard at St Andrews. His inaugural lecture, which was given in the United College Hall, dealt with atomic structure. He illustrated his subject, and electron spin in particular, by billiard balls. In 1930 he was elected a Fellow of the Royal Society. His elder brother, Edgar Johnson Allen, the Founder and Director of the Marine Biological Laboratory at Plymouth, who died in 1942, was already a Fellow.

Allen had a shy and retiring personality. His colleagues, both at King's College and at St Andrews noticed that. I was well aware of his ability as a teacher. His lectures were carefully prepared and beautifully illustrated by experiments. He lectured slowly, I believe, and his more intelligent students certainly derived much benefit from his teaching. It was not till he had left King's that I gradually began to recognize his greatness as a man of science. I have in my possession an interesting letter which he wrote to me on 11 December 1919. He had been attending some popular lectures on Einstein's theory of gravitation given by the Astronomer Royal for Scotland, Professor Sampson, and in his letter to me he writes: 'I have jotted down a few ideas which came to me, and am sending them to you in the hope that you may be able to collaborate in working them out. Perhaps they are all nonsense, but it does seem to me that a 5-dimensional manifold is required to represent the facts of physics'. I have now forgotten what precisely these notes expressed; but undoubtedly Allen must have had a pre-vision of some such mode of unification of gravitational and electrical phenomena as that published in 1921 by Kaluza. Unfortunately I had not then progressed far enough to appreciate the significance of his suggestions.

On arrival at St Andrews it fell to Allen to superintend the reconstruction

and enlargement of the Physics Laboratory and the result was, I believe, one of the best equipped laboratories in Great Britain. It was opened in 1925 by Sir William Bragg, F.R.S. Unfortunately it was burnt down soon afterwards; but Allen faced the disaster in characteristic fashion. He set to work, without fuss or excitement, and patiently directed its rebuilding.

At St Andrews he became interested in the band spectrum of hydrogen, his most active research student in this subject being Dr Ian Sandeman, who continued to correspond with him about it till comparatively recently. His most distinguished pupil was no doubt G. B. B. M. Sutherland, who was both a mathematician and physicist; but eventually devoted his efforts mainly to physics and is now Professor of Physics in the University of Michigan.

Allen began to be interested in the quantum theory and atomic phenomena while still at King's College, London. He took part in a discussion on the structure of the atom at the Royal Society on 19 March 1914, his contribution being a tentative suggestion that the core of an atom might be regarded as a revolving sphere of positive electricity, or, alternatively, *that the electron might be a revolving sphere of negative electricity*, a remarkable anticipation of the spinning electron which the experimental work of Uhlenbeck and Goudsmit suggested to them about eleven years later! It is only fair to recall here that Alfred Lauck Parson, now living at Allonby in Cumberland, suggested in 1915 that the electron might be a ring of negative electricity spinning with a high velocity about its axis.

While at St Andrews Allen wrote the following works: *The quantum and its interpretation* (1928), *Electrons and waves* (1932) and, in 1939, a textbook on *Heat* (in collaboration with his nephew, the Rev. R. S. Maxwell who, since 1947, has been Archdeacon of St Vincent and St Lucia in the West Indies). The *Quantum and its interpretation* is a most delightful book. It was written before Allen knew anything about Max Born's pregnant suggestion that the 'waves' in wave-mechanics are expressions of probabilities, or to put it in a better way, that the mathematical expression of the probabilities concerned in quantum mechanics has the form familiar to us in that of wave propagation. The most interesting and instructive chapter in this book is the final one entitled 'The interpretation of the quantum'. There is in it a most illuminating section devoted to the meaning of 'explanation' in physics. It ends by a quotation, the origin of which is not given. Natural philosophy, it is suggested, provides us with 'an image, in mind, of a reality previously existing'. It impresses me as very like the Platonic doctrine of reminiscence or recollection. In the section entitled 'The quantum' he is naturally unable to reconcile the conflicting views of waves and of particles; but makes the sensible suggestion that a more comprehensive theory will be found and remarks, quite correctly, that the work of de Broglie and Schroedinger is pointing the way towards such an all-embracing theory. Allen *was not a positivist*. In the concluding section of the chapter we are discussing he quotes with approval the words of the great Max Planck: 'Our aspirations after a uniform theory of nature,

on a mechanical basis or otherwise . . . can never be permanently repressed.' Allen's attitude to the quantum theory in 1928 is well set out in a supplement to *Nature* (8 December 1928) which contains the substance of a lecture delivered at St Andrews in October of that year.

In 1932 he had a severe attack of pneumonia after which his health began to fail; but in his earlier days he was an enthusiastic cyclist. He and his wife and family often spent their holidays abroad. It fell to him, while acting for the Dean of the Faculty of Arts to present the Duchess of York (now Queen Elizabeth, the Queen Mother) for the honorary degree of LL.D. His son Frank was seriously wounded in the battle of El Alamein and Allen and his wife suffered great anxiety for many weeks when they had no news from him. No doubt this contributed to Mrs. Allen's ill health and her death in 1943, before their son reached home. Allen retired in 1944 and the University conferred on him the honorary degree of LL.D. and the title of Emeritus Professor of Natural Philosophy. He continued to live in St Andrews and his daughter kept house for him till her marriage in 1953. The few remaining months of his life were spent with her and her husband in Ross-shire. His last days seem to have been very happy. He had the pleasure of knowing that his son Frank had become a successful farmer and of seeing both his children happily married. The arrival of his first grandchild, I am told, was a great joy.

It should be added, when describing the passing of this great man, that his religious faith remained firm till the end of his life and it seems fitting to close this, no doubt inadequate, account of his life and work by a sentence, referring to him, in the extract, which I have recently received, from the Minutes of the Meeting of the Senatus Academicus held on 29 June 1944 (drafted, I am informed, by another great man of St Andrews, Sir D'Arcy Thompson): 'Perhaps he does not realize how strongly he has endeared himself to his colleagues and his students by his own personality, his faith and vision, his simple friendliness and his single-hearted devotion to a life task.'

In writing this notice I have received much assistance from several correspondents. My grateful acknowledgments are due, more especially, to Allen's daughter, Mrs Walker; to Sir Edmund Whittaker, F.R.S., Professor H. W. Turnbull, F.R.S., and Dr Jack of the Department of Natural Philosophy at St Andrews.

WM WILSON

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Rafferty

RICHARD STAFFORD CRIPPS

1889-1952

‘At all times, a man who will *do* faithfully, needs to believe firmly.’

(Carlyle. *Lectures on heroes.*)

STAFFORD CRIPPS was a man who ‘believed firmly’ and ‘did faithfully.’ He was one of the few to whom the opportunity was given to match his great qualities with the needs of a great hour—a critical hour in the history of Britain. It was written of him when he died that he had been ‘for twenty years one of the most remarkable individual forces in British politics’*; but in his public life his culminating achievement—that which has had a lasting effect on the social and economic history of Britain and that by which his character can best be judged—was his conduct in the years after October 1947 when he became the first man to combine the offices of Minister of Economic Affairs and Chancellor of the Exchequer. All he did then was based on his intense feeling of the public need and his simple conviction that the nation could overcome its economic problems only by improvement in productive effort combined with restraint in expenditure. He expressed this conviction firmly and courageously in his policy, but above all, and this was his great personal achievement, he was able to impress the same conviction on the Trade Union leaders. He did more than any man to get across to the Trades Union Congress the realities of the national economic problems and to make Trade Unionists realize that the only sure way to improve earnings and standards of living was to increase the productivity of labour.

Stafford Cripps had the intellectual qualities which could have brought him fame as a scientist and which actually made him one of the great lawyers of his time. There are many people today who look back to his handling of their difficult cases as the most brilliant example of forensic ability that they have known. Men of science, too, were constantly astounded by the knowledge and appreciation of difficult scientific principles which he displayed both as counsel in patent cases and later in his work as Chairman of the Radio Research Board or as Minister of Aircraft Production and President of the Board of Trade. The distinctive feature, however, in his life’s achievement was his power of action—his single-hearted determination and courage in putting into action the course which his intellectual powers of analysis and logical thought had convinced him to be right. Intellectual

* *The Times* 22 April 1952.

thought or mere forensic argument without action could never have satisfied him. Favourite lines with which he ended one of his sermons were:

‘Knowledge we ask not—knowledge Thou has lent,
But, Lord, the will—there lies our bitter need,
Give us to build above the deep intent
The Deed—the Deed.’*

But for Stafford Cripps the idea of action alone was not enough. For him an essential tenet, constantly urged in his words and demonstrated by his conduct, was that action could not be rightly directed or effectively sustained without a spiritual urge inspired by religious faith. ‘The longer I live,’ he said in an address to the Federal Bar Association in Washington in September 1948, ‘and the more I see of the operation of these great material forces and powers, the more absolutely am I convinced that they can only be ordered for the good of mankind as a whole if the ordinary common man and woman—expressed in the highest form in the family group—exercises not some superintelligence, but that absolutely sound moral judgment which is dictated by his or her spirit. And I do not believe that without some form of personal religion that spirit can maintain its vitality and strength. As to what form that personal religion should take, we should no doubt many of us differ. For me it is essentially and inevitably Christianity.’ His Christian faith was of a clear almost childlike simplicity. And it had for him a pervading reality which influenced all that he did and said. He was, like any ordinary Englishman, shy of parading it, yet at times he was so moved by the urgency of our national problems and so convinced that without Christian faith no true solution could be found that he took courage (after much thought and communing with his wife) to speak of it even in the House of Commons. Speaking there on 23 October 1947, as Minister for Economic Affairs, he ended with these words: ‘I wish that today our country could refresh its heart and mind with a deep draught of that Christian faith which has come down to us over two thousand years and has, over those centuries, inspired the peoples of Europe to fresh efforts and new hopes. It is that spirit and not our own material hopes and desires that can be the most potent source of our inspiration, call it by what name you will, self-sacrifice, honour, love or comradeship; it is the strongest power in our lives and at this moment of deep difficulty in our history we need its supporting strength as never before.’

As to the sustaining power of the spirit, he had said earlier in the address to the Federal Bar Association which has been quoted above, ‘It is amazing what an imperfect body can do when it is inspired by the right spirit.’ Those words bear the stamp of his own experience, and point to what made him, apart from all his other qualities, remarkable as a man. His daily quota of work in his last years was astounding. His frail and failing body accomplished every day a task of sheer physical effort far beyond the powers of ordinary men. He was miraculously sustained by his spirit.

* *God in our work*, p. 26.

It has been said of him that he was 'a man with a mission' and that he displayed all the virtues and weaknesses of such a character; that his integrity, his unsparing devotion to the causes which he upheld, and his great courage, were balanced by impatience of the restrictions implicit in political action or of the necessity for awaiting the assent of minds which worked more slowly or seemed to him to see less far and clearly than his own. That may be true, but it was his integrity that prevailed. He could be driven by a passionate urge to fight against what he saw as social evils or as moral weakness in the conduct of national policy, and that helps to explain some of his conduct and violent speeches in his middle life which shocked large sections of public opinion. But what distinguished him was that in all that he did or said he was so clearly fighting for what he believed to be right with no taint of personal ambition or desire to gain publicity as a political careerist. Sir Winston Churchill truly said of him 'He strode through life with a remarkable indifference to material satisfactions or worldly advantage.'

This quality of selfless devotion to what he saw as his public duty came to be recognized by all who worked with him. Nothing was more remarkable than the way in which, when he was Minister of Aircraft Production, he won the confidence and affectionate admiration of leading industrialists who, on their interpretation of his earlier record, started by regarding him with suspicion and even hatred. One instance is worth recording. A certain prominent personality among the industrialists concerned with aircraft production went to the length of having reprints made of a cartoon from *Punch* published in the thirties and depicting Stafford Cripps making a violent speech of incitement to labour. These reprints he circulated widely among the industrialists with whom a Minister of Aircraft Production had to work. Stafford Cripps was greatly distressed by this; partly because, in spite of his apparent indifference to the opinion of others, he was sensitive and craved affection; but mainly because he feared that this kind of opposition might hamper his effectiveness in his task as Minister. What he did was to ask the man to come and have a frank talk with him, and in the end he made him one of his most sincere and affectionate admirers.

All this concerns his public life. No true assessment of his character is possible without going behind this into the inner circle of his family. That will not be attempted here. But this must be said. As an intellectual conception Stafford Cripps, to take his own words already quoted, saw human life as 'expressed in its highest form in the family group'. If that was what his head told him, his heart confirmed it in all his conduct and emotions. It was as a husband, a father or a brother that his true self was fully revealed.

Richard Stafford Cripps was born on 24 April 1889. He was the youngest of a family of five—four sons and one daughter. His father, Charles Alfred Cripps (later Lord Parmoor) was at the time of his birth and in all his early

years one of the most successful figures at the Parliamentary Bar and a Conservative—very conservative—Member of Parliament. Alfred Cripps came of an old Gloucestershire family. At the time of Stafford's birth he was a wealthy man with a large landed estate and a spacious country home at Parmoor on the Chiltern Hills. His fortune he had mainly made himself. He said that his income during the whole of his years in practice at the Bar had averaged over £10 000 a year, and at the height of his career he was, according to a letter of Beatrice Webb's, earning £1000 a week during the Parliamentary session. Stafford's mother was Teresa, the sixth of the nine famous Potter sisters of whom the most famous was Beatrice Webb. They were the daughters of Richard Potter of Standish Park, Gloucestershire, a lively and intellectual household in touch with scientific interests and with family friends like Herbert Spencer. Stafford inherited much of his mother's character, and her influence, surviving in strange ways after her early death when he was only four, was a powerful factor in his upbringing. She left behind her records of her thoughts about her children. 'I should like their living to be of the simplest, without reference to show or other follies. I should like them trained to be undogmatic and unsectarian Christians . . . taking their religious inspiration from the New Testament.'

But the influence of his father with his interests in the law and the Church was also a powerful one, and it is worth noting that the standard manual of Church Law was written by Stafford's grandfather, rewritten by his father and brought up to date again by himself.

Stafford Cripps thus grew up in surroundings which combined in an unusual way the atmosphere of Victorian county family security, pioneer intellectual thought, worldly success in law and politics, sincere religious thought and orthodox churchmanship. It is interesting to trace how these varying influences affected or failed to affect the pattern which he made for his own life.

The family at Parmoor was itself a remarkable one. There can have been few which showed so wide a range of individual variety combined with such a unity of family affection. It was a very happy family, and, for those who were friends of the elder brothers, holidays at Parmoor remain as golden memories of their undergraduate days. There was gaiety and sport. The brothers used to take over the Oxford University drag-hounds to Parmoor for the Christmas 'vac' and run lines which, led by Fred—one of the best and boldest riders to hounds of his time—provided fun and peril for all who cared to follow him. To this company such visitors as Beatrice and Sidney Webb provided strange, but stimulating contrast. Stafford was the baby of the family and, since his mother had died when he was so young, the elder ones tended to feel for him a protective affection tempered with a sense of awe at his intellectual powers, which grew with the growing record of his academic successes. He could take the lead in fun, but, though as a boy he loved riding, he never took great interest in field sports. Throughout his early years, without breaking the circle of family affection, he went his own way. And then

the baby of the family startled them all by getting married in 1911 when he was only 22.

This brief glance at his young home life reveals how, from the beginning, he marked out his own way. His academic career completes the picture. He went to Winchester—his father's and grandfather's school—in 1901 when he was twelve. H. M. Burge (later Bishop of Southwark and then of Oxford) was headmaster, and work with Burge provided one of the main formative influences in his life. The outstanding features of Burge's work at Winchester were that he made science a subject of serious intellectual discipline and not merely a refuge for the stupid or the idle: that he was a man of great personal austerity, combined with a singular humanity; and finally that, as his later life as Bishop showed, he had quite pronounced Liberal political leanings tending to Radicalism. He thought that there was something profoundly amiss in our social system requiring leadership with a large vision to seek a remedy. Stafford Cripps was in touch with Burge throughout Burge's later life, and it must have been more than coincidence that the three interests—science, religion and political outlook—should have been common to these two men of different generations.

From Winchester he won a chemistry scholarship at New College. It was natural for him as a Wykehamist who was the son and grandson of Wykehamists to think of going to New College; but in the end he made his own choice of a different course. Sir William Ramsay had been examining for New College and was greatly impressed by Stafford Cripps's papers. Stafford on his side was equally impressed by the scientific stature and personality of Ramsay. The prospect of working with Ramsay and doing research work in the Gower Street laboratories decided him to relinquish his New College scholarship and go to University College London. In his time there he developed qualities of leadership which he had already begun to show at Winchester. Soon after entering University College he was elected President of the Union and his contemporaries record that he ran the students' affairs with much advantage to the whole College. Also he made many friends, especially in the Chemistry Department, amongst them Alfred C. Egerton (Secretary of the Royal Society 1938-48) who later married his sister Ruth.

In his course as a student Stafford again marked out his own way. He read with intensity and, under the encouragement of Ramsay, he did some brilliant research work. Notably, he was part author at the age of 22 of a paper which was thought worthy to be read before the Royal Society on 'The critical constants and orthobaric densities of xenon'. But he left University College in his last year before graduating. He wanted to get on quickly with two purposes on which his heart was set—marriage and work at the Bar.

His marriage in 1911, already mentioned, was to Isobel, second daughter of Commander Harold Swithinbank. Then began a perfect companionship, never broken. Thereafter Stafford was, among all his activities, essentially a husband and head of his own family unit. No more will be said on that here,

except to add the brief record that he had one son and three daughters, that he added to the family two adopted members, the orphaned sons of friends, and that at first they made the family country home at the village of Filkins near Lechlade and later, after the children were grown up, at Frith Hill near Stroud.

Reading for the Bar and married life thus began together for Stafford. His eye had always been on the Bar. That was in a way natural to his father's son. But there was a more direct attraction than that. Work at the Bar suited his temperament. It gave him opportunity to exercise in a practical way his great intellectual powers of analysis and logical argument. For him it represented Aristotle's conception of *ἐνέργεια κατ' ἀρετήν*. He was called to the Bar in 1913 and started work in the chambers of H. A. Colefax, the leading specialist of that time in patent cases. But the start of work at the Bar was soon interrupted by war.

When war broke out in August 1914 Stafford Cripps felt at once that he must do something. He was medically unfit for the Army and went to France as a Red Cross lorry driver. In the following year he was called back for work in the Explosives Department of the Ministry of Munitions, and was for some time Assistant Superintendent of the Government factory at Queensferry. This practical experience of executive management in industry was one of which he took full advantage. He found it interesting, and it gave him a background of knowledge about what happened on the factory floor which influenced both his political views about labour relations and also his handling of his task when, in the Second World War, he became Minister of Aircraft Production. The experience, however, did not last long. After some months he became very ill. Overwork, and intestinal disease which he had contracted while in France laid him low. His condition for some time caused anxiety. He recovered sufficiently to do some light work at the Ministry of Agriculture, but again he had to give up and he was an invalid for the next two years. In all his life after that he had from time to time to fight against physical disability.

It was not until 1919 that he was well enough to get back to work at the Bar. The next years, in spite of occasional interruptions from ill-health, show a continuous record of success, success as a junior when he soon came to be appreciated for the thoroughness with which he mastered the smallest details of every case, success which received its first notable public confirmation in his handling of the Duff Development Company case, success which impressed the Colonial Office solicitor so that he began at once to give Stafford Cripps work in the Privy Council. Another notable forensic achievement was when he was briefed by the L.C.C. to enquire into railway charges. In 1926 he took silk and thus, at the age of 37, became the youngest K.C. This proved to be a successful step for him. In the years that followed he handled a series of big and complicated cases, and very soon he had built up one of the most lucrative legal practices of the day.

All this work gave him satisfying activity; but his thoughts were constantly

turning to the wider human and political problems which, as a man, had always interested him. Then once more he came close to his father, who had in the meanwhile broken his connexion with the Conservative Party and joined the new and rising Labour Party. It would be beyond the scope of this note to trace the causes of change in his father's political outlook. Here it may be said that he had become a pacifist during the 1914-18 War and then a moving spirit in the World Alliance to promote international friendship through the Churches. Stafford Cripps attached himself to this movement and, between 1923 and 1929, acted as Treasurer of the Alliance and made a number of speeches advocating its cause all over the country. Merely to preach and make speeches, however, was not enough to satisfy him, and in 1929, as the result of a direct approach from Mr Herbert Morrison, he made his entry into political life by joining the Labour Party. Hardly more than a year later when he was still only 41 he was made Solicitor General. A seat was found for him at West Bristol and, in January 1931, he was elected to Parliament with a solid majority. His first months in Parliament fulfilled his earlier reputation as an advocate, but he had little time before the crisis which split the Labour Party when Ramsay Macdonald decided to form a National Government. Stafford Cripps naturally moved to the Left and refused an invitation to serve in the new Government.

His entry into party politics in 1929 had marked the beginning of a new phase in his life. Till then he had been a brilliantly successful professional man, gaining satisfaction from exercising his intellectual powers and from doing each professional job as well as it could possibly be done. At the age of 40 he had built up a fortune and proved his power to earn a very large income and support his family. After 1929 he became essentially a fighter for causes. His profession became a secondary consideration and his capacity for earning money a means to provide financial aid to support the causes for which he fought. All that he did in the rest of his life can be interpreted as action for the cause which at the time he saw as of dominating importance.

In the first years after he entered political life the vitally important cause for him was the 'social' cause—the fight to counter the evils which he saw in Capitalism and Imperialism, to free the weaker classes or peoples from every form of exploitation and to redress inequalities of wealth. Seeing this as the supreme end, his logical mind thought out the steps which might help to attain it, and his will drove him to every form of effective action. When the danger of war first began to appear in the thirties he still saw the social objective as of prior importance. 'If war comes before the workers of Great Britain have won power', he said in one of his notable speeches in 1935, 'the war will be an Imperialistic war.' Accordingly his logical line of thought was—the Government needs the workers' labour to help in war production: now, therefore, is the time for the workers to assert their power. And that line of thought led him to make the speeches which shocked sections of public opinion. But as the years passed after 1935 and there became revealed the full nature of the threat from Hitler to all that Stafford Cripps himself held

to be of true value in human life, he began to see that another cause had become of more urgent and dominating importance. Speaking in the House of Commons in the Munich debate of 1938 he declared that the immediate need was 'strength to maintain the rule of law internationally'. Once again for him thought meant action. He felt that the Chamberlain Government would never provide the necessary strength. Therefore, he must use all his powers to fight for its removal and be ready to work with any people, whatever their political party, who would join in that fight. It was then that he started his campaign for the 'Popular Front', seeking 'the co-operation of every genuine anti-Government party or group of individuals'.

The bare record of these two stages in his thought and action reveals the strength of purpose which led him throughout his life to be ready to 'go it alone'—to go his own way to work for the cause which he thought was right, regardless of personal interest or conventional party allegiance. His action in these two stages brought him into conflict with the official Labour Party, first on one front and then on the other. In the first stage, when he was fighting for the 'social cause', he felt that the official party had neither the courage nor the plan for sufficiently radical action. So then he looked for new allies on the extreme Left: he advocated a 'British United Front of the Working Class' to defeat the National Government, and sought for that purpose to bring together in the 'Socialist League' the Labour Party, the Communists and I.L.P. This was too much for the Labour Executive. In March 1937 they declared that no member of the Labour Party could also be a member of the Socialist League and, shortly after, they laid down that any member of the Labour Party appearing on a public platform with any member of the I.L.P. or the Communist Party would be expelled automatically. Stafford Cripps argued strongly against this ruling at the annual conference of the Labour Party in 1937, but was heavily defeated. On this occasion, however, the difference did not lead to a break between him and the Labour Party: indeed at the same conference the Constituency Labour Parties re-elected him to the Executive. In the second stage things went differently. When, in that stage, he saw the crucial issue as one of getting 'strength to maintain international law' he was ready to look to the right as well as the left for new support. And this led to a real break. When he announced his programme for a 'Popular Front' campaign the Labour Party Executive asked him to withdraw it, and, when he refused, he was expelled from the Party.

The remarkable thing about these two incidents was the way in which his intrinsic integrity shone through and affected the attitude of his colleagues to him and his influence on them. He never was regarded as a dangerous rebel threatening to disrupt the party, never as one actuated by personal jealousies or ambition for attaining party leadership. And all the time, although the official party condemned his action, his opinions and conduct were greatly influencing its members.

War came quickly, and for a time Stafford Cripps, still an exile from the

Party, continued to pursue a lonely and courageous course. He felt he had personal powers which ought to be used in the national cause more effectively than would be possible merely by making speeches in the House of Commons from the Opposition Benches. With the sympathetic support of Lord Halifax, who was then Foreign Secretary, he set out by himself just before the end of 1939 on a world tour. He went to India, Burma, China, Russia and the United States.

This gave him a double opportunity, on the one hand to gain up-to-date knowledge of conditions and thought in those countries and, on the other hand, to explain the spirit and purpose which were moving the British people to fight the war. The visits to Russia and India led directly to important later episodes in his life.

The Russian episode came first. He had always felt the supreme urgency of keeping Russia from becoming an active ally of Germany in the war. He thought, after his visit to Moscow, that his own personal influence could help towards this end, and on his return to England in May 1940 he offered to go out on a special mission. His return from his world tour coincided with the change in the British Government. Domestic political divisions had faded into insignificance before the national emergency. The Labour Party had joined the National Government under Winston Churchill and the difference between the Labour Party and Stafford Cripps had lost all meaning (though he was not officially re-admitted to membership of the Party till much later). On 20 May he was asked by Churchill to go as British Ambassador to Moscow. Four days later he was off again, flying on his way. But his time in Moscow was not a happy one. His immediate practical objective, to keep Russia from joining Germany in the war, was, of course, soon achieved by Hitler's attack on Russia. But he found no fulfilment of his hope that he, with his left-wing outlook, might be able to make an effective personal contribution in Anglo-Russian relations by creating understanding and sympathy with Britain. Stalin wanted British material help in fighting Hitler: he was not interested in discussing Socialist ideology with Stafford Cripps. And so Stafford found himself stranded in isolation, forced to report after a few months that 'there is no development of any sort in our relations with the Soviet Government'. In fact the practical arrangements for mutual assistance and military aid were made through other agencies and he returned finally to England in January 1942.

Then began a new phase in his life. Somehow or other, in spite of his frustrations in Moscow, he had during his absence become a national figure of intriguing significance. He, therefore, had an expectant audience when he gave a broadcast talk on his return. This was one of the most successful efforts in his public life. What he said, and above all the way in which he said it, made a deep impression. Thereafter everyone knew for what he stood and had a sense of his power as a personality. It was clear that he must be given a prominent place of some kind in Britain's National Government. The question was what place? Winston Churchill's answer to that question was a sur-

prising one. He asked him to become leader of the House of Commons with the office of Lord Privy Seal and a place in the War Cabinet. And then once again Stafford Cripps found himself cast for a role for which he was not really suited.

The record of the events which followed and of his own behaviour shows more clearly than anything else in his life his moral stature, his integrity and his strength as a personality. It was clear from the first that he had not the peculiar qualities required for success as a leader of the House of Commons. No one who knew him would have expected it—or thought the worse of him for that reason. It was said of him in *The Times* obituary notice that ‘he had a habit of scolding the House and speaking to it rather as a schoolmistress might to a disobedient class, which caused considerable irritation’. That is perhaps too unkind, and does not tell the whole story; but it sufficiently indicates one side of the matter. However that may be, there can be no doubt that the arrangement was not a happy one and he himself was not sorry to interrupt it. The state of public opinion in India was becoming a cause of anxiety. In the political field the planned course of constitutional advance had been interrupted by the War. In the economic field the British Government was being forced to make increasing use of Indian material resources to help the war effort, and, although this involved incurring heavy debts to the Indian Government from the repayment of which that country was in the end to reap great benefit, the immediate effect had to be a drastic control of internal consumption which caused much discontent. Stafford Cripps once again felt that he had a personal contribution to make and volunteered to go out on a personal mission. His most intimate political associates thought he was very foolish to do this: that he was ‘sticking his neck out’; that he was making his characteristic mistake of over-estimating the power of his personal influence: that he did not realize that in politics the individual could do nothing unless he had a party behind him, etc., etc. Perhaps, according to the standards of ‘Mr Worldly-Wiseman’, they were right. Certainly no immediate tangible results came from his 1942 visit to India.* But the notable thing is that all these apparent failures fell harmlessly off his shoulders. He continued to ‘stride on through life’ doing public work of first-class importance and rising in the end to a position of power and influence which towered above that of the political ‘Worldly-Wisemen’ who had thought him so foolish.

The immediate happenings on his return were an acid test for him. It was clear to all—and not least to himself—that it would be a mistake to re-insert him into his post as Leader of the House of Commons. Churchill asked him to become Minister of Aircraft Production without a place in the War Cabinet. From a worldly point of view this was a hard thing. It clearly meant a step down and a mark of disapproval, or at least difference, from

* It should be added that, although there was no immediately visible practical result, there are many who were in the inner circle of Indian affairs in those days who say that this visit served to increase Indian esteem for Stafford Cripps and thereby in the long run helped in creating the new understanding between Indian and British statesmen.

the Prime Minister. He must have been greatly tempted to break away and resume his old position of independence. But once again his conduct was governed by his conception of the supremely important cause. That cause at that moment was obviously national survival through strength in war. Aircraft production was of dominant importance. The post offered direct scope for action. So Stafford accepted the post and thereby gained a new respect from all who understood what was involved. Also, still more important, he entered then upon that final phase of his life in which for ten years he held vitally important executive posts, posts involving intense work and heavy responsibility, posts which gave him the opportunity by his own power to influence the destiny of the nation. From that date the man who had been spending his energy in urging others to act became himself one of the most important instruments of public action and remained so until his final physical collapse.

From the first he had great success as a Minister of Aircraft Production. Reference has already been made to the way in which he ultimately won the confidence and affection of the leading industrialists with whom he had to work. His success with his own staff within the Ministry was more immediate; and today he has left behind him none who look back to his character and work with more sincere admiration than those who in official posts worked closely with him at the Ministry. Moreover the impress of his personality and his winning of personal affection was not confined to those with whom he worked closely at the top. At the Ministry of Aircraft Production and later at the Board of Trade and the Treasury, he became the friend and revered leader of all—messengers and liftmen, typists and clerical workers of all grades. He won affection by giving it and showing it in his daily conduct: he won respect and loyalty by his own example. And since this achievement reveals one of his most notable characteristics, which was felt with increasing force throughout his years as a Minister, it is appropriate at this point to include an appreciation by one of his own leading officers at the Treasury.

‘In nothing did Stafford Cripps show himself more completely than in his conception of the duties of a Minister in charge of a large Department. Constitutionally, of course, a Minister is responsible for everything done by his Department; but nearly all Ministers are content to leave the day-to-day management to their senior officials and to leave it to their advisers to bring to their notice the matters which they ought to handle personally.

‘Not so Stafford Cripps. He was determined that everyone in the Department should share his own burning enthusiasm about the true purposes of the work of the Department and of the way in which that work should be carried out.

‘This was one of the reasons for his practice of holding morning meetings with his senior advisers. But he also made it a practice on occasions to address his whole staff, or as many as could be collected together in some large hall, about the work of the Department.

‘Then again he felt for every member of his staff, whether senior or junior,

whether personally known to him or not, what can be only described as a truly pastoral care. He wanted to know and share the joys and sorrows of all. He was not content to be less than in spirit the father of his whole great official family: and he insisted on being told if any member of his staff had some bereavement or affliction, and, even at his busiest, on writing a letter of condolence in his own hand.

'No one had a higher standard of how work ought to be done, or set a finer example by his own selfless devotion to the work in hand. And if someone fell lamentably short of his own high standards, no Minister was ever more apt at letting his disapproval take the form of regret that one of his friends and colleagues should do something which was unworthy of the Department or of service to the country. And few combined so much of high seriousness of purpose with so good a sense of fun.

'One at least of those who worked with Stafford Cripps in his later years felt that until he had known Stafford he had not realized what management and leadership of a Department could be.'

At M.A.P. he achieved speedy action by cutting through all the friction of official formalities. But this he did in a clear well-organized way and without bringing back that hectic atmosphere of improvisation which had—perhaps necessarily in the early days—characterized the regime of Lord Beaverbrook. His introduction of the procedure which came to be known as 'morning prayers' was typical of his methods. Under that arrangement all his leading officers sat round a table with him every morning to keep each other informed of progress in their different fields of work and to exchange ideas. This brought the whole complex range of activities into orderly co-ordination so that the left hand knew what the right hand was doing. These meetings took place at 9 a.m. by which time Stafford himself had normally put in something like four hours work in reading the necessary daily pile of papers. He had the capacity and courage to give immediate decisions and everyone who knew how to present the relevant facts clearly could rely on getting a decision after five minutes exposition. He listened, he trusted his officials and, once a decision had been taken, there was no wavering or hesitation: he backed his men and saw it through. His unequalled powers of analyzing the factors in any case and grasping the essential points which had been developed in his practice at the Bar realized their full value in this work. He was helped, too, by his scientific education and by the practical experience of factory management which he had gained in his work for the Ministry of Munitions in the 1914-18 War. His early training as a scientist was specially valuable. This had given him an appreciation of the scientific outlook, both in relation to research and also in the application of the scientific method of approach to all practical problems. The scientists who were in close touch with him felt this and he was able to get much assistance from them.

His experience at M.A.P. in working with private industrial concerns in the field of aircraft production undoubtedly influenced his views on the relationship between Government and private enterprise—a relationship which

became of such significance as a political issue when the Labour Government came to power in 1945. He showed that he could trust and welcome the co-operation of private firms so long as they were doing their job efficiently and in the national interest. But if they were inefficient or failing to co-operate fully in the national production programme set by the Government, the logical conclusion for him was that they must be taken over by the Government. There were hard cases for decision. Stafford Cripps was not unsympathetic. He did not fail to recognize the tremendous difficulties with which the aircraft industry had to cope as a result of the expansion of production and dispersal of factories which the requirements and conditions of war had made necessary. These steps had meant that family concerns, accustomed to work on a comparatively small scale at one centre, found themselves responsible for many factories separated by hundreds of miles, with a labour force of hundreds expanded to tens of thousands and with foremen from the home factory, accustomed to the charge of some fifty men, being of necessity promoted to management of works employing hundreds or even thousands.

Most of the pre-war aircraft companies coped well with these problems. A few, however, failed. Failure endangered the national interest and the Minister was placed in a difficult position. The mere act of taking over a private firm was easy enough; the difficulty was to find the men to provide more efficient direction and management. Stafford Cripps had several hard problems of this kind to tackle. He always tackled them with quick decision and courage. That often involved unpleasant human problems. Yet, in spite of the pressure of work, he always found time to see and deal sympathetically with the individuals affected. But inefficiency or personal motives which took no account of the national need he could not tolerate.

He remained Minister of Aircraft Production till the General Election in 1945, and then, on the return of the Labour Government, he became President of the Board of Trade. He brought the same spirit of courage and efficiency to this work. Once again he concentrated attention on what his logical analysis showed him to be points of key importance. Once again he got together a loyal team working for him including several men specially appointed from outside the regular Civil Service. And here again, as at M.A.P., his early training as a scientist was of great value and helped him to get the best out of his experts. Sir Thomas Merton (Treasurer of the Royal Society) was one of his scientific advisers at the Board of Trade; Sir Ian Heilbron, Sir William Stanier, Professor Blackett and many others worked in close touch with him. (At this point it is also appropriate to record that he was the guest of honour at the Anniversary dinner of the Royal Society in 1946*, and that he was elected as a Fellow in 1948 under Statute 12 'as a person who had rendered conspicuous service to science'.)

As usual he went straight to the heart of the problem and concentrated on that. The elementary truth as he saw it was that, if Britain was to survive, an

* His speech on that occasion is given in full in *Notes and Records of the Royal Society*, 5, 5, published October 1947.

immense increase in productive efficiency was necessary. Only thus could we meet the burden of war debts, overcome the handicap of the sacrifice of foreign investments, repair war damage, make good the arrears of house building and other capital expenditure which had piled up during the war, and, finally—to the Labour Party most important of all—make a start on the road to better social services and higher standards of living. Not only must the volume of production be adequate, but the goods produced must be of a quality and price which could be sold in competitive export markets to provide the means of buying essential imports. An increase in the volume of exports by 50 to 75 per cent over the pre-war volume was necessary. Stafford Cripps put all his strength into working for the required improvement in productive efficiency.

What he did can be seen as inspired by certain main conceptions. First the general public, and especially the great body of workers, must be made to understand the realities of the national economic problem and its desperate urgency. Secondly there must be a spirit of co-operation between management and workers, based on the understanding and right handling by management of all the factors which affect good human relations in industry. Thirdly there must be a drive for improvement in the methods and techniques of production—improvement in the art of management and in forms of organization, improvement in industrial processes and equipment. This third requirement he saw as having value not only for the direct purpose of increasing technical efficiency, but also as helping towards the second—the spirit of co-operation. ‘Co-operation’, he often said ‘depends on confidence and confidence depends on competence’. Fourthly, since the task which lay immediately before the nation was to build up foundations of strength for the future, it would be necessary, during this process of building up, to accept a regime of austerity and restraint in consumption in which the Government must give a lead.

His life in the next years contains an impressive record of effective work towards all these ends, and the influence of what he did then is surviving today in many ways. But for him that alone was not enough. Behind all these practical concepts there lay the dominating idea that the greatest need of our times was that men should be animated by a spiritual force and a spiritual understanding which looked beyond mere material objectives. He felt in fact that we could not achieve a true and worthy national purpose without a great movement of moral regeneration based on acceptance of Christian principles and drawing spiritual strength from prayer. Without this, not only should we fail to sustain the co-operative effort needed for success in our material tasks, but—ininitely more important—success would not be worth winning if men did not see the true purpose of human life as something more than mere material gain.

These concepts provided for Stafford Cripps the foundations for a perfectly integrated course of conduct and policy. He was not harassed by any conflict between worldly purposes and the other-worldliness of Christ’s teaching. This

was not because he ignored the possibility of conflict. A constantly recurring theme in his later addresses and sermons was 'the essential duality of our nature—the body of man and the Spirit of God for ever competing for the control of our life and destiny'. But he had very clear ideas as to how this conflict should be resolved, and the way for him was certainly not escape from or disregard of, worldly tasks. 'It is wholly false to imagine that we were created to be hermits or recluses. That may be the role that some should play . . . but for the great majority of the human race it is our task to live as social beings, to blend our personality into the domestic, local and national group. We can thus form a great co-operating social unit, working together for the development of human civilization for the achievement of the Kingdom of God on earth. The needs of the body must be served as well as those of the spirit. We must feed and house and warm ourselves and others—that is part of our inevitable responsibility as human beings. We do not live by bread alone, but without bread we cannot live, and certainly it is no part of the Divine plan that we should hurry through this world to our early death in discomfort, poverty and disease.' But the great danger, as he saw it, was that material things of life should be given undue importance and so 'weigh us down and oppress us'. 'Where we and the world fail today' is that 'our spirits have been unable in the conflict of our dual natures to control our material instincts, and the darkness of human fear has obliterated the light of divine love.' In his view there was only one way to counter this failure. We must have *real* faith in our religion, not merely regarding it 'as some form of rather doubtful insurance against possible misfortune in the next world', and, further, we must realize that if we try to follow that faith in our daily lives 'we shall not have the strength and courage to go through all the difficulties that we must encounter unless we constantly refresh ourselves with the spirit and love of God'.

The quotations just given are from a speech made in Edinburgh in February 1948—just at a time when Stafford Cripps was struggling with his most acute problems as Minister of Economic Affairs and Chancellor of the Exchequer. His words were simple and familiar: what was remarkable was that for him, carrying an overwhelming burden of worldly responsibilities, the spirit of these words represented the dominating reality in his life and the source from which he drew his own strength for his daily task.

The record of his performance in that daily task is one of clear vision, courage and purposive action which not only brought him to a dominating position in the Government, but made a permanent mark on the nation's history. In the first years of the Labour Government the natural post-war conditions of boom had been intensified by Dr Dalton's 'easy money' policy and had created a dangerously inflated appearance of prosperity. Stafford Cripps alone saw clearly the realities of our underlying dangers, or at least he alone had the courage to proclaim them and to advocate unpopular policies necessary to meet them. Gradually, as the difficulties came to light, his influence on public opinion grew and public confidence in him increased.

In October 1947 a new post was created for him as Minister of Economic Affairs, and then a few weeks later, on Dr Dalton's resignation, he was transferred to the Treasury and enabled to play the dual role of Minister of Economic Affairs and Chancellor of the Exchequer. This was a development of historic significance. For a long time it had been clear that one member of the Government ought to be responsible for the general guidance of economic policy and that no one could do this effectively if the responsibility for fiscal and monetary policy was in other hands. Fears had been felt of giving too much power to the Treasury, and various ideas had been suggested for creating a super-Ministry of Economic Affairs with the Treasury subordinate to it. But the Treasury had built up by long tradition such a dominant position of authority, and was already responsible for manipulating so many levers in the economic and financial machine, that such an arrangement would almost certainly have involved friction and confusion. It was at this moment that the qualities of Stafford Cripps were of decisive importance. His intellectual ability, integrity and courage, combined with the confidence which he had by now inspired, made him by common consent so well fitted for the post that its creation became an obvious step just because he was there to fill it.

The new post gave him greatly increased powers and scope. He was now concerned not merely with action in the front line of production: he could go behind that and by his fiscal and monetary policy influence all the conditions which affected the development and daily working of industry. Yet his attention remained concentrated on what he saw as the 'main cause'—the improvement of productive efficiency. He remained primarily a 'production' rather than a 'finance' minister.

For an appreciation of his character and his life's achievement it is instructive to consider not only his executive decisions on the issues which arose from day to day during this culminating period of his public responsibility, but also, and still more, the general spirit in which he worked, the movements which he initiated, and the instruments of organization which he devised. For this was what really distinguished him as a Minister: he was always ready to take quick, courageous decisions about immediate practical problems, but all the time he was looking ahead and working to create the spirit and forms of organization which might fit the country to cope with its continuing problems in the difficult and changing conditions of our times.

His influence, which was working for the same broad purpose both before and after he went to the Treasury, can be traced in many forms and along many channels. Some see its most important and distinctive effect in the mere fact that he by his personal character and ability was able, as the first holder, successfully to combine the two posts of Chancellor and Minister for Economic Affairs. Thus one man who worked closely with him during these years has said, 'It seems to me that of the organizations and machinery which Stafford Cripps left behind far and away the most important is the fact that the Chancellor of the Exchequer is now accepted without question as the Minister responsible for economic policy as a whole. No one who had not

tried to deal with economic policy in the days when the Minister charged with that responsibility had no control over the Chancellor of the Exchequer can imagine a tithe of the confusion and frustration that existed. . . . I do not think that this particular fact can be stressed too much.'

Apart from this, Stafford Cripps started many organs and lines of activity, some within the sphere of Government, such as the Economic Planning Board, others outside it, such as the British Institute of Management, the 'Working Parties', and the Anglo-American Council of Productivity.

The Economic Planning Board proved to be a useful instrument. It still continues in its previous form, though rightly with much less impact on affairs than it inevitably had in times of crisis. The British Institute of Management has become a live and valuable organization, financed partly by Government grant, but mainly by subscriptions from its industrial members and supporters. It was sponsored by Stafford Cripps on the recommendation of a Committee which he appointed under the Chairmanship of Sir Clive (now Lord) Baillieu. Its work has been one of the factors contributing to the marked change which has taken place during the last ten years in the attitude of British industrial leaders.

The other two moves are of special interest because they so clearly illustrate the ideas which inspired Stafford Cripps. The 'Working Parties' (of which there were fourteen) were set up to examine the structure and methods of certain of the older British industries which, for various reasons, seemed specially to need intensive study in order to gain realistic understanding of what measures were needed to make them progressive and prosperous in the changed and difficult post-war conditions. The purpose of the Anglo-American Council of Productivity—an imaginative conception worked out by Stafford Cripps in collaboration with Paul Hoffman of the U.S.A.—was to arrange visits by representative teams from British industries to the United States for studying methods of working in American industry and its standards of productive efficiency in terms of output per man, quality of product, etc.

Both these schemes had one important feature in common—the studies were to be undertaken jointly by representatives of 'both sides' of industry. The Working Parties were made up of four representatives of employers, four of employees, four 'outside' members with varying expert qualifications, together with an independent Chairman. The teams which visited the United States under the auspices of the Anglo-American Council of Productivity combined equal numbers chosen by the employers and Trade Unions respectively in each industry.

Judged by external results the Anglo-American Council of Productivity has proved to be a much more live and effective project than the Working Parties. Yet of the latter it may be said that they helped to create a general understanding of the structure of the industries which were examined and that this has been one of the contributory factors to that growing consciousness of the importance of improving productivity in British industry which

has been such a notable feature in the last ten years and which Stafford Cripps more than any other man helped to arouse. The development of the Anglo-American Council of Productivity has been remarkable. More than fifty teams of British representatives have visited America, and provision has now been made for the permanent continuance of this kind of work by the setting up of a British Council of Productivity. The work of the teams has had three valuable features. In the first place—as already noted—they got representatives of the ‘two sides’ of industry working together in a new way and above all gave Trade Union representatives an opportunity to understand what was necessary to improve the productivity of their industry on which the chances for improving earnings and working conditions must ultimately rest. Secondly the planning of these visits helped to break down the secretiveness of individual British industrial concerns and to encourage a readiness to exchange ideas which was entirely new. In all cases, as a preliminary to the visit to America, the teams made a review, based on visits to a number of factories, of the whole industry which they represented. Thirdly, the visits to America provided a great educative influence. They made even the most conservative people realize that they had something to learn, they gave lessons of real value, and they provided British industrialists with some sort of yardstick against which to measure their own achievements.

Another line of work to which the support of Stafford Cripps gave an effective impetus has been the study of all the factors affecting human relations in industry. His interest took many forms, including the provision of grants to finance a programme of research into human factors affecting productivity which was promoted by one of the four panels of the Government Committee on Productivity. The continuance of this work has now been provided for by the setting up of a Committee under the joint supervision of the Medical Research Council and the Department of Scientific and Industrial Research.

In various ways such as these Stafford Cripps helped to create pieces of administrative machinery or to start lines of activity which have proved to be of continuing value. At the same time he was making his influence felt in more general ways which were of wider significance. Reference has been made in the opening paragraphs to what he did to convince the Trade Unions of the economic needs of the country and of the necessity for increased productivity to meet them. This was his constant effort during all his work after the war, and his most decisive success was achieved after he became Chancellor when in 1948 he was invited to address the Trade Union Congress at Margate. He had always been frank with the Trade Union leaders and the frankness of some of his earlier speeches had aroused considerable hostility. For example in a speech at Bristol in October 1946, dealing with industry, he had stressed the lack of men on the Trade Union side who had experience and knowledge about what was required for the efficient running of industry. ‘I have on many occasions’, he said, ‘tried to get representatives of the workers on all sorts of bodies and working parties. It has always been extremely difficult to get enough people qualified to do that sort of job. Until

there has been more experience by the workers of the managerial side of industry I think it would be impossible to have worker-controlled industry in Britain, even if it were on the whole desirable.'

Apart from straight talk of this kind, his campaign for the freezing of wages had been by no means popular. Yet in his speech at Margate he won a convincing triumph, and got across to the whole body of the Trade Union Congress an understanding of the country's economic position which has had a lasting effect. As usual he had taken an immense amount of trouble. Before the meeting he had prepared an ordered statement of the facts on which he would base his case, and copies of this statement were distributed to all the delegates in good time before the meeting. In presenting his case and in handling the discussion afterwards he showed the mastery of facts and clarity of exposition which had been the foundation of his success at the Bar. In the result he secured the endorsement of his policy and won a personal respect for which there can hardly be a parallel.

All that has been written up to this point has concerned work on the home front. But in the conditions which have prevailed since 1945 a British Minister for Economic Affairs has been almost equally engaged in the international field. As to this the following note has been written by one who worked closely with him in all his international economic discussions.

'One facet of the many fruitful activities of Stafford Cripps is perhaps less known than it should be, and that is the great effort he made in connexion with the post-war reconstruction of Europe which involved the most delicate and complicated negotiations both with Western Europe and the United States.

'The Marshall Plan has proved to be, by common consent, one of the most successful post-war international enterprises. To this great co-operative scheme he made a contribution second only to that of Ernest Bevin, to whose prompt realization of its possibilities, both political and economic, Europe owes so much.

'Stafford Cripps devised financial techniques and methods of international co-operation which were entirely novel at the time and have now become, almost imperceptibly, part of the recognized machinery of international collaboration. At the same time he used his brilliant powers of exposition and advocacy to explain the needs of Europe in lucid and compelling terms to the Truman Administration. His efforts greatly helped that Administration to obtain from Congress, only partly conscious of the grave deterioration in the European fabric, the vast sums which were so generously voted in the early years of the Marshall Plan. It was only the efficient use of these vast sums through machinery which Cripps took a great part in directing, which rendered possible the revolutionary increase in European production and productivity which are now matters of history.

'The task of bringing the whole of Western Europe to accept solutions which involved serious internal political difficulties for many of the Governments concerned, in order to secure advantages for Europe as a whole, was

not an easy one. Wide vision, great strength of character, a readiness to break new ground in methods of international collaboration, and inexhaustible patience and good humour were all necessary; these Stafford Cripps provided in full measure.

'To watch him in Paris, often after a strenuous morning in the Treasury in London, exercising, far into the night, his gifts of lucid exposition, good humoured persuasion and remarkable financial ingenuity, was an experience which none who were privileged to take part will ever forget. It was a constructive and stimulating period in which the countries of Western Europe slowly struggled to their feet by self-help and mutual aid geared to generous American financial assistance. All three were necessary for Europe to survive, and Stafford Cripps's contribution in original thought and administrative skill was recognized by all participants as quite outstanding.

'Two remarks made by colleagues of his, which were given wide circulation at the time, may be worth recalling now.

'The first was made by Maurice Petsche, the French Minister of Finance, whose warm friendship Cripps gained and held in spite of many hard tussles at the conference table and fundamental differences in political outlook:

"'Stafford n'est past seulement un grand Anglais, il est un grand Européen: je dirai même un grand homme tout court.'"

'The second came from Paul Hoffman, a great American industrialist who, as head of the American Economic Co-operation Administration, displayed gifts of statesmanship and broad humanity for which Europe has cause to be grateful.

"'Leave it to Stafford. He will find a way out. If only some of our political leaders at home knew his qualities they would be less terrified of Socialism.'"

'Stafford Cripps is mourned by many in this country. He is also mourned by many in wider international circles with whom he worked as Chancellor of the Exchequer, who learned to regard him with admiration respect and even affection. It is rare for an Englishman in his position to be held in such high esteem abroad, and this esteem was well deserved.'

It has been right to recount these achievements, because, as has already been said, they throw more light on the character and work of Stafford Cripps than could be gained from following out a detailed record of how he worked out his financial and economic policy. No attempt will be made here to set out such a record. There is room for differences of opinion on the broad issue of whether his line of policy was right; but none can dispute that when once the line of policy had been chosen he showed a measure of courage and consistency in its execution which is rare in the history of democratic politics. So far as concerns the 'broad issue', this note is written in the belief that when it is possible to review the events of those years in their proper perspective it will be generally acknowledged that this country owes Stafford Cripps a great debt of gratitude for his policy of austerity and for the measure of success which he achieved in getting ideas of restraint in wage claims and personal expenditure for a time accepted. In his ideas of fixing targets for production

and prescribing detailed schemes for the distribution of raw materials he may have attempted a precision which went too far; but at the time when he took over economic responsibility the national economic body had not sufficiently recovered from the fractures and dislocations of war to make it safe to remove all the splints and bandages of controls, and such success as has been achieved in recent years by the relaxation of controls may largely have been made possible by the foundations which were consolidated during the regime of Stafford Cripps.

This, too, can be said of him. If every man and woman in the country had thrown themselves into their work with the spirit of selfless devotion which he showed, he would have accomplished all his purposes. Some may say that he was expecting too much of human nature; but that would not be true. As has already been noted, he felt that full success in our national tasks could not be achieved without a process of fundamental moral regeneration. All the time he preached for that and set his sights high. He would have been wrong to do anything less. In the end he had to face certain failures, above all the failure to maintain Britain's position in international trade without devaluation of sterling. This must have been an agonizing experience for him. He had to do what other Finance Ministers have had to do before him—assert to the last moment that devaluation was unthinkable, and then suddenly announce it as an accomplished fact. No other course is possible for the responsible Minister in such circumstances; but only those who have been through or near to such an experience can know what it means.

At this date, looking back to what happened then, it can be seen clearly that this devaluation was inevitable and that it has proved beneficial to the country. Moreover, it is difficult to see how the awkward operation of transition could have been better handled. But the mental strain took heavy toll of Stafford Cripps's strength, and from then onwards he was fighting the final phase in his long struggle against physical disability. In all his years of office, and particularly as a leading member of the Labour Government after 1945, he had been over-taxing his strength. He had not only carried the normal heavy responsibilities of his office: he had taken on other duties as well, such as the Cabinet Mission to India with Pethick Lawrence and Alexander in 1946, during which he became extremely ill.

There can be few who so clearly worked themselves to death in public service. He was forced to resign from the Government on 20 October 1950, completely exhausted. And then after months spent in a private clinic in Switzerland he died on 21 April 1952.

It is too early yet to assess the lasting results of the influence of Stafford Cripps; but there can be no doubt that there are many things happening today which would be happening differently or not at all if he had not lived and worked as he did. All statesmen who hold responsible positions must by

their normal executive acts in some measure affect the course of history. That certainly is true of Stafford Cripps; but beyond this he has left his mark, not only on the methods and machinery of Government, but on the whole spirit of the public services and on ways of thought among his fellow men.

The attempt has been made in this note to give a broad picture of his influence—but there is one aspect which has not been mentioned. Nothing in modern times, or, indeed, in the whole history of Britain and the Commonwealth has been more notable than the way in which friction and ill-feeling between an Imperial power and a subordinate people have in the case of India been converted into friendship and collaboration between equals. The spirit and conduct of Stafford Cripps as a representative British statesman contributed to this result as much as that of any other man. It is interesting to note that out of the subscriptions to a Stafford Cripps Memorial Fund which has been raised in the last two years more than one-fifth has come from India and Pakistan and that the appeal for subscriptions in India was personally sponsored by Mr Nehru.

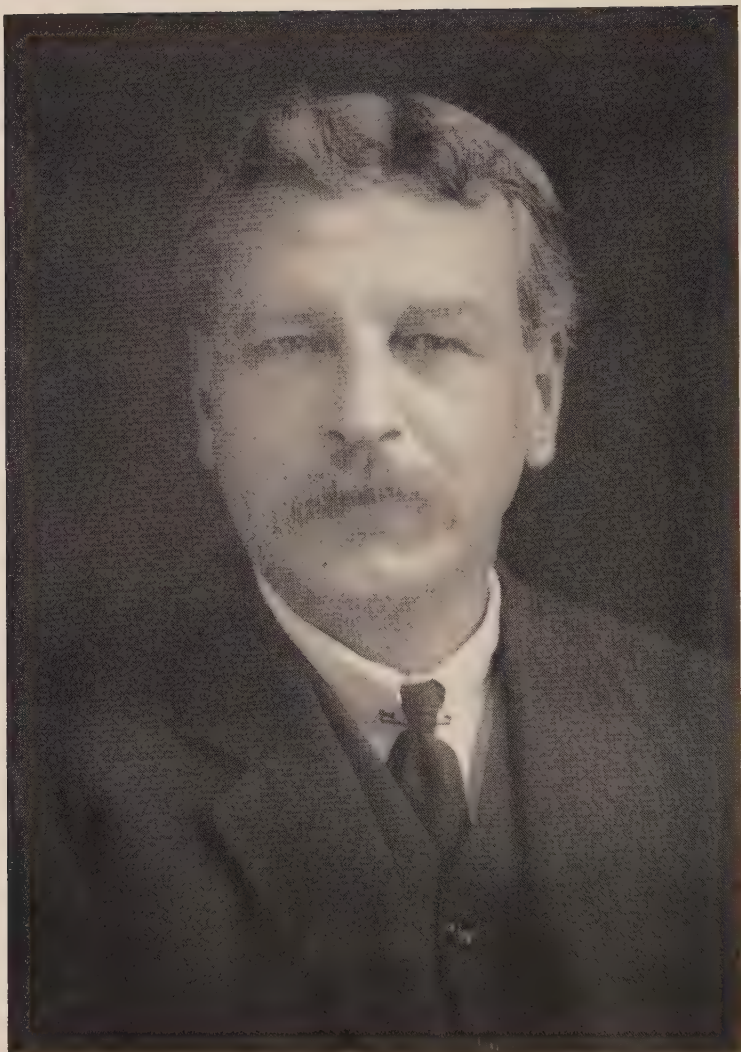
In all these ways Stafford Cripps has left his mark; but, in the end, if the judgment of his character which has been given in this note is correct, he himself would have thought most of his influence in the field of moral and spiritual values, especially through his conception of work as service to God, with 'men working together as a great co-operating social unit for the development of human civilization and the achievement of the Kingdom of God on earth', devoting themselves to their material tasks as part of a worthy human life, but not becoming obsessed with material values.

And here surely he has exercised a wide and lasting influence by his example as a sincere Christian who proclaimed and acted on his belief that Christ's teaching is relevant to the problems of our modern industrial society and must provide guidance for our daily actions. Today in many industrial centres there are groups of men getting together in attempts to work out clear practical ideas as to how Christian principles should be interpreted and applied in all the complex human relations which are involved in the working of industry. For many such men the words and example of Stafford Cripps have helped to show the way and provided strength for faith and hope.

This note began with a quotation from Carlyle: it may suitably end with one, since his words ring true for Stafford Cripps. 'If a man has to ask at every turn the world's suffrage; if he cannot dispense with the world's suffrage and make his own suffrage serve, he is a poor eye-servant.' Stafford Cripps gave true service, supported not by the world's suffrage but by his own conscience and his faith in God.

GEORGE SCHUSTER

(The photograph of Sir Stafford Cripps is by Karsh of Ottawa.)



A. L. Dixon.

ARTHUR LEE DIXON

1867-1955

ARTHUR LEE DIXON was born at Pickering, Yorkshire, on 27 November 1867, the second of three sons of the Rev. G. T. Dixon, Wesleyan minister of Northallerton. His elder brother, A. C. Dixon, was also a distinguished mathematician, and a Fellow of this Society (see *Obituary Notices*, **2**, 165 (1936)). A. L. Dixon was educated at Kingswood School, Bath (1879-85) and Worcester College, Oxford (1885-9), where he was a mathematical scholar. He was a Fellow of Merton College for the thirty-odd years 1891-1922, at first a Prize Fellow and then a Tutorial Fellow. He became Savilian Professor of Pure Mathematics in 1922 and held this chair until his voluntary retirement in 1945. He was elected into the Royal Society in 1912, and was President of the London Mathematical Society in 1924-5.

Dixon was married in Paris in 1902 to Catherine, eldest daughter of Léon Rieder. The climate of Oxford proved disastrous to Mrs Dixon's health and she was compelled to spend much of her married life outside Oxford, in Pau and elsewhere. There was one daughter of the marriage, who became Mrs F. J. Baden Fuller; after his wife's death in 1930 it was with his daughter and her husband, in Sandgate, Kent, that Dixon made his home (a very happy home), and it was here that he died on 20 February 1955 in his eighty-eighth year.

He was a man of the sunniest disposition, radiating *bonhomie*, welcome in every company and with a wide circle of friends. He was gentle in manner, somewhat reserved in speech, with a quiescence that was never to be thought inertia. He shunned controversy and, possibly because he lacked aggression, he also lacked leadership. It was a mark of the esteem in which he was held by his colleagues that, in 1945, on his voluntary retirement from his professorship and, therefore, from his fellowship at Magdalen College (for he held the chair before a retiring age was imposed), he was elected a 'supernumerary' fellow without emolument, but with full rights to rooms and dinner.

Dixon had none of the remoteness of the conventional mathematician. His eye was alert and served him well in his various ball games: hockey, tennis, squash rackets, croquet. In some of these he retained his powers well on in life. In his prime he had great physical strength and, it was said, could crack a walnut in the hollow of his arm.

His ear, too, was sensitive. He was both a good linguist and a skilled musician. He played the flute for many years in an Oxford orchestra and with his

friends, his taste leaning to the contrapuntists, as might be expected of a mathematician.

He was perhaps the last link in Oxford with the mathematics of the Victorian age. He acknowledged his indebtedness to the inspiration of E. B. Elliott, and he had a story to tell of meeting C. L. Dodgson (Lewis Carroll) as a mathematician. His first published work and indeed much of his later work could be described as in the tradition of Cayley. He was then principally interested in algebra (in the older sense) and particularly in algebra as applied to geometry and in the algebra (if one may use the word) of the elliptic and hyperelliptic functions; in this latter theme he retained his interest through most of his active life. In all this work he showed a mastery of algebraic form, with a quick eye for the underlying symmetries and the power of exhibiting them convincingly.

In 1908 he began to publish a noteworthy series of papers on algebraic eliminants, advancing a subject that had been left incomplete by Cayley. In particular he was interested in the definition of polynomials by interpolation instead of through their coefficients, and he exploited this usefully in his presentation of eliminants.

After a longish silence he returned to an earlier study of problems connected with the cubic surface—the lines on the surface, the double-six, the Schur quadric and the like—and published a number of papers on these topics. They overlapped a distinguished series of nearly a dozen papers written in collaboration with W. L. Ferrar and published in the *Quarterly Journal of Mathematics*, then newly taken over by the Clarendon Press. This series dealt with problems in the analytic theory of numbers such as summation-formulae and lattice-point problems and with associated integrals in the theory of Bessel's functions. They exhibited Dixon's powers as an analyst, never wholly dormant, it is true, for he had from time to time published short notes on problems of analysis; but as a distinct break from the bulk of his previous work these papers do represent a remarkable achievement for a mathematician aged between sixty and seventy. His last paper, a brief note on a measure of distance, was published when he was in his eightieth year.

He had no great sympathy with much of the mathematics now in vogue. Matrices, he agreed, meant something; but so often in modern writing, when one had mastered a notation and terminology that were unfamiliar (and, you suspected, repellent), one discovered it was something one had known all along.

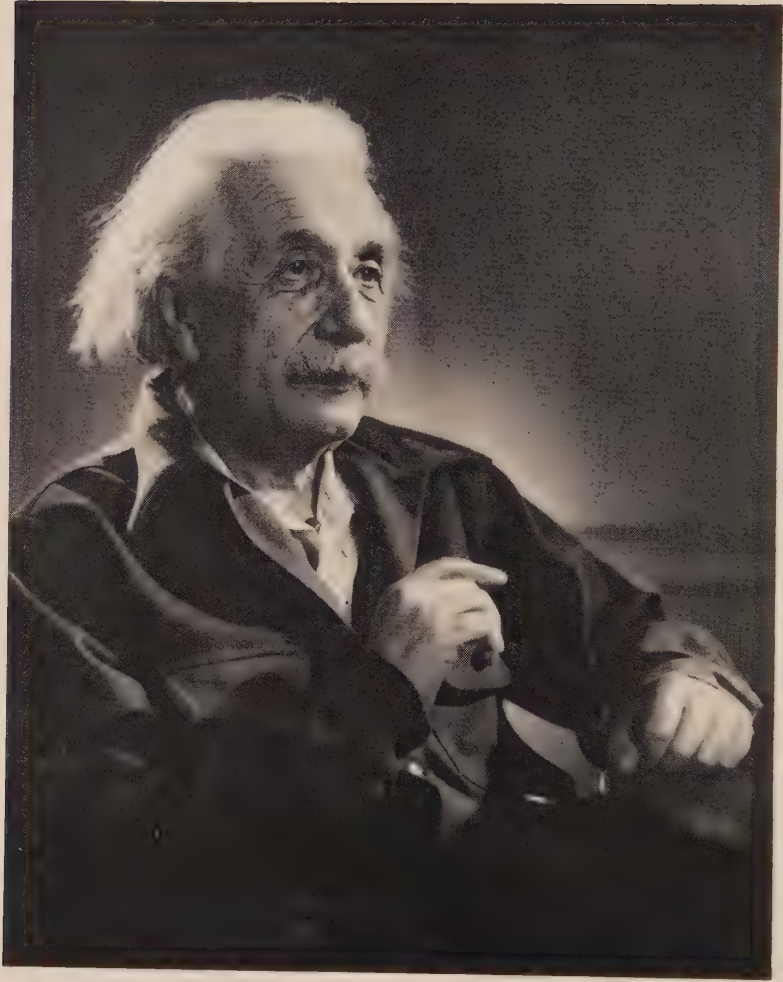
Like many dons of his generation Dixon never wrote a book. His work and teaching are enshrined in his papers published in a few English journals, in the inspiration he gave to his pupils, and in the affection of his friends, who remember him as one who met life and mathematics with a bright and unflinching equanimity.

T. W. CHAUNDY

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A. Einstein.

ALBERT EINSTEIN

1879-1955

ALBERT EINSTEIN was born on 14 March 1879, at Ulm in Wurttemberg, Germany, the son of Hermann Einstein and his wife, Pauline, née Koch. Hermann Einstein was in partnership with his brother, an engineer, in a small factory producing electrical supplies. From an autobiographical fragment* we learn that the Einstein parents were Jewish, but non-observant of their religion; at a very early age the boy's religious nature became dissatisfied with the spiritual emptiness of his surroundings: seeking for something deeper, he attached himself ardently for a time to the faith of his fathers; but further reading led him to the opinion that fact cannot easily be separated from legend in the framework of Jewish history, and he ceased to accept Judaism as a transcendental religion, while retaining its humanitarian principles. In later life he was a keen Zionist and a Governor of the Hebrew University of Jerusalem.

Meanwhile, his education was somewhat irregular, owing chiefly to changes in domicile brought about by unsatisfactory circumstances in his father's business. Between the ages of 10 and 15 he attended the Luitpold Gymnasium at Munich. Later for a year he became a pupil of the cantonal school at Aarau in Switzerland, and at the age of 17 he entered the Technische Hochschule at Zurich, each successive move involving some discontinuity in the curriculum. Thrown very much on himself, he read a great many popular books of science and general knowledge, and was deeply impressed by the principle of the reign of law in the universe. This now became the centre of his religion. 'I believe', he declared, 'in Spinoza's God, who reveals himself in the harmony of all being, not in a God who concerns himself with the fate and actions of men.' The conception of a God who controls the grand mechanism of the world, but is indifferent to the petty affairs of human beings, recalls the system of Aristotle, in which the Prime Mover causes the rotation of the heavenly spheres, but takes no interest in their inhabitants. This view induced in Einstein what he calls a 'cosmic religious feeling', that is, a belief in the possibility of interpreting nature in terms of a mathematical system of great beauty and simplicity. This is the spiritual background of his mature intellectual achievements. By 1901 Einstein had completed his studies at Zurich Polytechnic School and had taken the step of becoming a Swiss citizen. He then obtained a position in the Patent Office in Berne, and married a lady, Mileva Maric, who had been a student with him at Zurich. She was a native of Hungary, of Serbian race, and nominally

* Printed in *Albert Einstein: Philosopher-Scientist*, edited by Paul Arthur Schlipp (Evanston, 1949).

of the Greek Orthodox religion. Two sons, Hans Albert and Eduard, were born to them on 14 May 1904 and 26 July 1910 respectively: they were baptized as Greek Catholics, but educated without reference to any particular religion.

In the years 1901 to 1904 Einstein published some investigations on molecular forces and thermodynamic principles, in particular, two papers (3) and (4)*, in which he obtained independently certain results on the statistical kinetic theory of heat which had been published a year or two earlier by Willard Gibbs. In 1905, (7), continued in (11), he applied these results to the motion of very small particles suspended in a liquid. The particles were supposed to be much larger than a molecule, but it was assumed that as a result of collisions with the molecules of the water, they acquire a random motion, like that of the molecules of a gas. The average velocity of such a suspended particle even in the case of particles large enough to be seen with a microscope, might be of observable magnitude, but the direction of its motion would change so rapidly, under the bombardment to which it would be exposed, that it would not be directly measurable. However, as a statistical effect of these transient motions, there would be a resultant motion which might be within the range of visibility. Einstein showed that in a finite interval of time t the mean square of the displacement for a spherical particle of radius a is

$$\frac{RTt}{3\pi a\mu N},$$

where R is the gas constant, T the temperature, N is Avogadro's number, and μ is the coefficient of viscosity. Thus, by this phenomenon the thermal random motion, hitherto a matter of hypothesis, might actually be made a matter of visible demonstration.

The motion of small particles suspended in liquids had been observed as early as 1828 by Robert Brown, a botanist, after whom it was called the *Brownian motion*. Einstein identified the motion studied by him with the Brownian motion, somewhat tentatively at first, but without hesitation in (11). The direct confirmation of the kinetic theory provided by studies of the Brownian movement was the means of converting to it some notable former opponents, such as Wilhelm Ostwald and Ernst Mach.

In the same volume of the *Annalen der Physik* as (7), Einstein, now aged 26, published two other papers which attracted unusual attention. In the first of these, (6), he supposed monochromatic radiation of absolute temperature T , frequency ν or c/λ and small energy density ρ (within the range of values of ν/T for which Wien's formula of radiation

$$\rho = \frac{8\pi ch}{\lambda^5} \exp\left(-\frac{hc}{k\lambda T}\right) d\lambda$$

is applicable where h is Planck's constant and k is Boltzmann's constant) to be

* The numbers refer to the bibliography at the end of this notice.

contained in a hollow chamber of volume v_0 with perfectly reflecting walls, its total energy being E : and, investigating by use of Wien's formula the dependence of the entropy on the volume, he found for the difference of the entropies when the radiation occupies the volume v_0 and when it occupies a smaller volume v , the equation

$$S - S_0 = \frac{Ek}{h\nu} \ln \frac{v}{v_0}.$$

Now, by inverting the Boltzmann-Planck relation

entropy = $k \times$ logarithm of probability,

he calculated the relative probability from the difference of entropies, and found that the probability that at an arbitrarily-chosen instant of time, the whole of the energy of the radiation should be contained within a part v of the volume v_0 , is

$$(v/v_0)^{E/h\nu}.$$

This formula he studied in the light of a known result in the kinetic theory of gases, namely that if a gas contained in a volume v_0 consists of n molecules, the probability that at arbitrarily-chosen instant of time, all the n molecules should be collected together within a part v of the volume, is

$$(v/v_0)^n.$$

Comparing these formulae, he inferred that the radiation behaves as if it consisted of $E/h\nu$ quanta of energy or *photons** each of amount $h\nu$. The probability that *all* the photons are found at an arbitrary instant in the part v of the volume v_0 is the product of the probabilities (v/v_0) that a single one of them is in the part v ; which shows that they are completely independent of each other.

Now, the quantum theory had originated in the year 1900, when Max Planck had asserted that a vibrator of frequency ν can emit or absorb energy only in multiples of $h\nu$. Planck at that time regarded the quantum property as belonging essentially to the interaction between radiation and matter: free radiation he supposed to consist of electromagnetic waves, in accordance with Maxwell's theory of 1861-4. Einstein in this paper put forward the hypothesis that parcels of radiant energy of frequency ν and amount $h\nu$ occur not only in emission and absorption, but that they have an independent existence in the aether.

It was shown by several writers within a few years that Einstein's hypothesis leads not to Planck's law of radiation but to Wien's, at any rate if it is assumed that each of the light quanta or photons of frequency ν has energy $h\nu$ and that they are completely independent of each other. In order to obtain Planck's law, it is necessary to assume that the elementary photons of energy $h\nu$ form aggregates, or photo-molecules as we may call them, of energies $2h\nu$, $3h\nu$, . . . , respectively, and that the total energy of radiation is distributed on the

* The word *photon* was actually introduced much later, namely by G. N. Lewis, *Nature, Lond.*, 18 December, 1926; but it is so convenient that we shall adopt it now.

average, in a regular manner between the photons and the different kinds of photo-molecules.

Einstein applied his idea in order to construct a theory of photo-electricity. In 1899 J. J. Thomson and P. Lenard had shown independently that a metal irradiated by ultra-violet light emits negative electrons; and in 1902 Lenard, continuing his researches, had shown that the number of electrons liberated is proportional to the intensity of the incident light, so long as its frequency remains the same, and that the initial velocity of the electrons is altogether independent of the intensity of the light, but depends on its frequency.

In Einstein's paper of 1905 (6) he asserted that when a metal surface is illumined by radiation, the radiation consists of parcels of energy; when one such parcel or photon falls on the metal, it may be absorbed and liberate a photo-electron; and that the maximum kinetic energy of the photo-electron at emission is $(h\nu - e\phi)$, where ν is the frequency of the light, and $e\phi$ is the energy lost by the electron in escaping from its original location to outside the surface. This, of course, implies that no photo-electrons will be generated unless the frequency of the light exceeds a certain 'threshold' value $e\phi/h$.

Einstein's equation was verified experimentally in 1912-16. For many metals, the threshold frequency is in the ultra-violet; but for the electro-positive metals, such as the alkali metals, it is in the visible spectrum: for sodium, it is in the green. The discoveries of this paper, and its sequel (12), were named as the chief grounds for the award of the Nobel Prize to Einstein in 1922.

We now pass to (8), the third of the famous papers of 1905. Natural Philosophy, in the eighteenth and nineteenth centuries, was founded on Newton's laws of motion. According to the First Law, any particle which is free from the action of impressed forces, moves, if it moves at all, with uniform velocity in a straight line. But in order that this statement may have a meaning, it is necessary to define the terms *straight line* and *uniform velocity*: for a particle which is said to be 'moving in a straight line' in a terrestrial laboratory would not appear to be moving in a straight line to an observer on the sun, since he would perceive its motion compounded with the earth's diurnal rotation and her annual revolution in her orbit. We can, however, define a straight line *with reference to a system of axes* $Oxyz$ as the geometrical figure defined by a pair of linear equations between x, y, z , and we can assert as a fact of experience that certain systems of axes $Oxyz$ exist such that free particles move in straight lines with reference to them. Moreover, we can assert that there exist certain ways of measuring time such that the velocity of free particles along their rectilinear paths is uniform. A set of axes in space and a system of time-measurement, which possess these properties may be called *an inertial system of reference*.

In Newtonian mechanics, if S is an inertial system of reference, and if S' is another system such that the axes $O'x'y'z'$ of S' have any uniform notion of pure translation with respect to the axes $Oxyz$ of S , and if the system of time-measurement is the same in the two cases, then S' is also an inertial system

of reference; the Newtonian laws of motion are valid with respect to S' just as with respect to S . No one inertial system of reference can be regarded as having a privileged status, in the sense that it could properly be said to be fixed, while the others were moving; so long as mechanics only is considered, Newtonian physics does not involve the notion of the absolute fixity of a point in space.

But physics comprised also optics; and the undulatory theory of optics, which was universally accepted in the second half of the nineteenth century, involves the doctrine of a medium, the *aether*, which permeates all space. Apart from its small vibrations, which constitute light, the aether must be supposed to be at every place in some state of motion or rest; and thus the question arose, what motion has the aether relative to (say) the moving earth at any point?

In the later years of the nineteenth and earlier years of the twentieth century experimental attempts were made to answer this question, based on physics as it was then understood, but all were unsuccessful: and in his lectures at the Sorbonne in 1899, Henri Poincaré said 'I regard it as very probable that optical phenomena depend only on the *relative* motions of the material bodies, luminous sources, and optical apparatus concerned'; in other words, he believed that absolute motion is undetectable in principle, by either dynamical or optical means. In the following year, at an International Congress of Physics held in Paris, he elevated this into a new principle introduced into physics, which would resemble the Second Law of Thermodynamics, inasmuch as it asserted the *impossibility of doing something*: in this case, the impossibility of determining the velocity of the earth relative to the aether. In a lecture to a Congress of Arts and Science at St Louis, U.S.A., on 24 September 1904, Poincaré gave to a generalized form of this principle the name *The Principle of Relativity*. 'According to the Principle of Relativity,' he said, 'the laws and physical phenomena must be the same for a "fixed" observer as for an observer who has a uniform motion of translation relative to him'; so we have not, and cannot possibly have, any means of discerning whether we are or are not carried along in such a motion.

We must now enquire how an analytic scheme can be devised which will enable physics to be reformulated in accordance with Poincaré's Principle of Relativity. In particular, we must learn how observers who have uniform motions of translation relative to each other can express the laws of the electromagnetic field in the same form, a discovery that was made in 1903 by Lorentz.*

Denoting by $\mathbf{d}$ the electric vector, by $\mathbf{h}$ the magnetic vector, and by c the velocity of light, the fundamental equations of the aether in empty space may be written

$$\begin{aligned} \operatorname{div} \mathbf{d} &= 0, & c \operatorname{curl} \mathbf{d} &= -\partial \mathbf{h} / \partial t \\ \operatorname{div} \mathbf{h} &= 0, & c \operatorname{curl} \mathbf{h} &= \partial \mathbf{d} / \partial t \end{aligned}$$

* *Proc. Acad. Sci. Amst.* (English ed.) (1903) 6, 809.

Now consider the transformation of the co-ordinates t, x, y, z which is represented by the equations

$$\begin{aligned} ct &= ct_1 \cosh \alpha + x_1 \sinh \alpha, \\ x &= x_1 \cosh \alpha + ct_1 \sinh \alpha, \\ y &= y_1, \quad z = z_1, \end{aligned} \quad (1)$$

where α denotes a constant parameter. The origin of the (t_1, x_1, y_1, z_1) system has at the instant t the co-ordinates $x = ct \tanh \alpha, y = 0, z = 0$, so it is moving along the axis of x with the constant velocity $c \tanh \alpha$. Moreover, transform $\mathbf{d}, \mathbf{h}$, to new variables $\mathbf{d}_1, \mathbf{h}_1$, according to the equations

$$\begin{aligned} d_x &= d_{x,1}, & h_x &= h_{x,1}, \\ d_y &= d_{y,1} \cosh \alpha + h_{y,1} \sinh \alpha, & h_y &= h_{y,1} \cosh \alpha + d_{x,1} \sinh \alpha, \\ d_z &= d_{z,1} \cosh \alpha - h_{x,1} \sinh \alpha, & h_z &= h_{z,1} \cosh \alpha + d_{y,1} \sinh \alpha \end{aligned} \quad (2)$$

Then in terms of these new variables the differential equations of the aether become

$$\begin{aligned} \text{div}_1 \mathbf{d}_1 &= 0, & c \text{curl} \mathbf{d}_1 &= -\partial \mathbf{h}_1 / \partial t_1, \\ \text{div}_1 \mathbf{h}_1 &= 0, & c \text{curl} \mathbf{h}_1 &= \partial \mathbf{d}_1 / \partial t_1, \end{aligned}$$

that is to say, *the equations in terms of the new variables have the same form as the original equations*. In this transformation (1) the variable x plays a privileged part, as compared with y or z . We can, of course, at once write down similar transformations in which y or z plays the privileged part; and we can combine any number of these transformations by performing them in succession. The aggregate of all the transformations so obtained, combined with the aggregate of all the rotations in ordinary space, constitutes a *group*, to which Poincaré* gave the name the group of *Lorentz Transformations*.

Einstein in (8) adopted Poincaré's Principle of Relativity (using Poincaré's name for it) as a new basis for physics and showed that the group of Lorentz transformations provided a new analysis connecting the physics of bodies in motion relative to each other. Notable results appearing in this paper for the first time were the relativist formulae for aberration and also for the Doppler effect.

In a later memoir, (9), of the same year Einstein discussed another consequence of relativity theory. In 1881 J. J. Thomson had shown that an electrically charged spherical conductor moving in a straight line behaves as if it had an additional mass of amount $(4/3c^2)$ times the energy of its electrostatic field.† In 1900 Poincaré, referring to the fact that in free aether the electromagnetic momentum is $(1/c^2)$ times the Poynting flux of energy, suggested that electromagnetic energy might possess mass density equal to $(1/c^2)$ times the energy density: that is to say, $E = mc^2$ where E is energy and m is mass.

* *C.R. Acad. Sci., Paris*, **140**, 1504 (5 June 1905).

† It was shown long afterwards by Fermi that the transport of the stress system set up in the material of the sphere should be taken into account, and that when this is done, Thomson's result becomes additional mass = $(1/c^2)$ energy of field.

In (9) and also in some later papers (13), (19) and (20) Einstein supported this conclusion, which both in atomic physics and in astrophysics has proved to be of immense importance.

The papers of 1905 made such an impression that Einstein was widely recognized as a man worthy of exalted academic position, and after becoming a *Privatdozent* at the University of Berne he was in 1909 appointed 'Professor Extraordinary' of Theoretical Physics in the University of Zurich, and in the autumn of the following year, Professor of Theoretical Physics in the German University of Prague. This was one of the two parts, German and Czech, into which the ancient University of Prague had, in 1888, been divided, for political reasons.

Einstein was now continually trying to remove certain imperfections which he considered to be present in the Theory of Relativity as it had existed in 1906; and in 1907 and 1911 he published papers, (20) and (41), in which he introduced what he later called the *Principle of Equivalence*, which may be thus described: Consider an observer who is enclosed in a chamber without windows, so that he is unable to find out by direct observation whether the chamber is in motion relative to an outside world or not. Suppose the observer finds that any object in the chamber, whatever be its chemical or physical nature, when left unsupported, falls towards one particular side of the chamber with an acceleration f which is constant. The observer would be justified in putting forward either of two alternative explanations to account for this phenomenon. (i) He might suppose that the chamber is 'at rest', and that there is a *field of force*, like the earth's gravitational field, acting on all bodies in the chamber, and causing them if free to fall with acceleration f ; or (ii) he might explain the observed effects by supposing that *the chamber is in motion*, if he postulates that in the outside world there are co-ordinate axes (C) relative to which there is no field of force, and if he moreover supposes the chamber to be in motion relative to these axes (C) with an acceleration equal in magnitude but opposite in direction to f ; then it is obvious that free bodies inside the chamber would have an acceleration f relative to the chamber.

The observer has no criterion enabling him to tell which of these two explanations is the true one. If we could say definitely that the chamber is at rest, then explanation (i) would be true, while if we could say definitely that the axes (C) are at rest, then explanation (ii) would be true. But by the Principle of Relativity, we cannot give a preference to one of these sets of axes over the other: we cannot say that one of them is moving and the other at rest; and we must, therefore, regard the two explanations as equally valid or, in other words, must assert that a homogeneous field of force is equivalent to an apparent field which is due to the accelerated motion of one set of axes relative to another—a *uniform gravitational field is physically equivalent to a field which is due to a change in the co-ordinate system*.

In (20) Einstein also showed, by combining Doppler's principle with the principle of equivalence, that a spectral line generated by an atom situated

at a place of very high gravitational potential, e.g. at the sun's surface, has, when observed at a place of lower potential, e.g. on the earth, a greater wavelength than the corresponding line generated by an identical atom on the earth. This may be shown very simply as follows. If we denote by Ω the gravitational potential at the sun's surface, the energy lost by a photon of frequency ν in escaping from the sun's gravitational field is Ω times the mass of the photon, or $\Omega h\nu/c^2$. Remembering that the energy $h\nu$ is hc/λ , we see that the wavelength of the solar radiation as measured by the terrestrial observer is $1 + (\Omega/c^2)$ times the wavelength of the same radiation when produced on earth.

In 1911 Einstein followed up this work by (41), in which he argued that *since light is a form of (electromagnetic) energy, therefore, light must gravitate,* that is, a ray of light passing near a powerful gravitating body such as the sun, must be curved: and the velocity of light must depend on the gravitational field.*

At this time also he made an important contribution (43) to his photon theory of light, by extending it to photochemical theory. Developing a suggestion made by Stark in 1908, he now asserted that when chemical action takes place under the influence of light, a molecule being dissociated as a result of absorbing radiation of frequency ν , the amount of energy absorbed by the molecule is $h\nu$.

His life was also affected by another academic change: he was offered and accepted a Chair of Theoretical Physics in his Alma Mater, the Polytechnic School at Zurich and he removed to Zurich in the summer of 1912.

In 1912-3 a number of theories of gravitation were published by Abraham, Einstein, (44), (45), (47), (48), Nordstrom and Mie; the decisive step forward was taken in 1913 by Einstein when he adopted a method of characterizing the state of space near a point, which was familiar to differential geometers and had been used in physics in 1909 by Harry Bateman.† According to Bateman, the condition that a luminous disturbance originating at the space point (x^1, x^2, x^3) at the instant x^0 , should arrive at the space point $(x^1 + dx^1, x^2 + dx^2, x^3 + dx^3)$ at the instant $(x^0 + dx^0)$ could be expressed as the vanishing of a quadratic differential form which may be written

$$\sum_{p,q=0}^3 g_{pq} dx^p dx^q \quad (1)$$

when the coefficients g_{pq} are functions of (x^0, x^1, x^2, x^3) . This form (1) is invariant for *all* transformation of the co-ordinates (x^0, x^1, x^2, x^3) : and its coefficients g_{pq} are characteristic of the physical properties of space near the point.

In empty space free from all forces the quadratic differential form (1) reduces to

$$c^2(dt)^2 - (dx)^2 - (dy)^2 - (dz)^2, \quad (2)$$

where c is the velocity of light, since the vanishing of this expression is the

* On this, cf. Planck, *S.B. preuss. Akad. Wiss.* (1907) p. 542.

† *Proc. Lond. Math. Soc.* (2) **8**, 223 (1910).

condition that a luminous disturbance originating at the point (x, y, z) at the instant t should arrive at the point $(x+dx, y+dy, z+dz)$ at the instant $(t+dt)$.

It had been shown in the later years of the nineteenth century by Ricci and Levi-Civita that when the physical conditions are such that the vanishing of the quadratic differential form (2) must be replaced by the vanishing of the more general differential form (1) then the differential calculus for operations in this space is replaced by a more general calculus called the *absolute differential calculus* or *tensor-calculus*. During his stay in Prague Einstein developed a close friendship with a mathematician, named Georg Pick, who expressed the opinion that the mathematical instrument needed for the further development of relativity theory was precisely *tensor calculus*; and Einstein now saw how this idea could be carried out. For, writing

$$(ds)^2 = c^2(dt)^2 - (dx)^2 - (dy)^2 - (dz)^2,$$

we know that the rectilinear motion of a particle free from all forces in empty space is determined by the equation

$$\delta \int ds = 0,$$

where δ is the symbol of the calculus of variations. Einstein now, with the help of his old friend Marcel Grossmann in Zurich, (51), (54), (55), (63), (65), put forward the theory, that similarly the motion of a free material particle in any gravitational field is determined by the equation

$$\delta \int ds = 0,$$

where

$$(ds)^2 = \sum_{p,q=0}^3 g_{pq} dx^p dx^q$$

is Bateman's quadratic form.

This was a tremendous innovation, because it implied the abandonment of the time-honoured belief that a gravitational field can be specified by a single scalar potential function: instead, it proposed to specify the gravitational field by the ten functions g_{pq} which could now be spoken of as the *gravitational potentials*. Einstein justified* this new departure by showing that the theory of a single scalar gravitational potential led to unacceptable inferences. He compared, for instance, two systems, in the first of which a movable hollow box with perfectly-reflecting walls is filled with pure-temperature radiation, while in the second the same radiation is contained inside a fixed vertical pit which is closed at the top and bottom by movable pistons connected by a rod so as to be always at a fixed distance apart, the pit walls and pistons all being perfectly-reflecting; and he showed that on the

* In §7 of (51).

single-scalar-potential theory the work necessary to raise the radiation upwards against the force of gravity would in the second system be only one-third of the work required in the first system, a conclusion which was obviously wrong. He admitted, however, that in his own mind the strongest reason for rejecting the single-scalar-potential theory was his conviction that relativity in physics exists not only with respect to the Lorentz group of linear orthogonal transformations, but with respect to a much wider group.

The ten coefficients g_{pq} not only specify the force of gravitation, but they determine also the scale of distance in every direction and the rate of clocks. The metric defined by

$$(ds)^2 = \sum_{p,q=0}^3 g_{pq} dx^p dx^q$$

is not, in general, Euclidean; and since its non-Euclidean qualities determine the gravitational field, we may say that *gravitational theory is reduced to geometry*, in accordance with an idea expressed by FitzGerald* in 1894 in the words 'Gravity is probably due to a change of structure of the aether, produced by the presence of matter.' The 'aether' of FitzGerald was called by Einstein simply 'space' or 'space-time'; and FitzGerald's somewhat vague term 'structure' became with Einstein the more precise 'curvature'. Thus we obtain the central proposition of the Einsteinian theory: 'Gravity is due to a change in the curvature of space-time, produced by the presence of matter.'

In comparing FitzGerald's statement with Einstein's, it may be remarked that if we consider a gravitational field which is *statical*, i.e. such as would be produced by gravitating masses that are permanently at rest relative to each other, then feeble† electromagnetic phenomena taking place in it can be shown to happen exactly in accordance with the ordinary Maxwellian theory of electromagnetic phenomena taking place in a medium whose specific inductive capacity and magnetic permeability are aetotropic and vary from point to point.

It is possible that when FitzGerald said 'Gravity is probably due to a change of structure of the aether', he was actually thinking of a change which would show itself in alterations of the dielectric constant and magnetic permeability, and that he had in mind an electrical constitution of matter, on account of which matter would be subject to forces depending on the values of the dielectric constant and magnetic permeability: by analogy with the fact that in a liquid whose dielectric constant varies from point to point, an electrified body moves from places of lower to places of higher dielectric constant.

What differentiates the Einsteinian theory from all previous conceptions is that the older physicists had regarded gravity as merely one among many types of natural force—electric, magnetic, etc.—each of which influenced in its own way the motion of material particles. Space, whose properties were

* FitzGerald's *Works*, p. 313.

† i.e. so feeble that they do not appreciably change the curvature of the field.

set forth in Euclidean geometry, was, so to speak, the stage on which the forces played their parts. But in the new theory gravity was no longer one of the players, but part of the structure of the stage. A gravitational field consisted essentially in a replacement of the Euclidean properties by a much more complicated kind of geometry: space was no longer homogeneous or isotropic. An analogy may be drawn from the game of bowls. Bowling-greens, in the north of England, are not flat, but rise to a slight elevation in the centre. An observer who failed to notice the central elevation would find that a bowl (supposed without bias) always described a path convex toward the centre of the green, and he might account for this by postulating a centre of repellent force there. A better-informed observer would attribute the phenomenon to a geometrical feature—the slope. The two explanations correspond respectively to the Newtonian and the Einsteinian conceptions of gravity: for Newton it is a force, for Einstein it is a modification of the geometry of space.

When the metric of space-time is specified by an equation

$$(ds)^2 = \sum_{p,q=0}^3 g_{pq} dx^p dx^q,$$

an observer moving in any manner will have a *world-line* consisting of the points of space-time which he successively occupies; and at any point of his world-line he will have in his immediate neighbourhood an *instantaneous three-dimensional space*, formed by the aggregate of all the elements of length which are orthogonal to his world-line at the point, orthogonality being defined by the statement that two vectors (X) and ($\mathcal{I}$) are said to be orthogonal if

$$\sum_{p=0}^3 X_p \mathcal{I}^p = 0,$$

where (X_p) is the covariant form of one vector and ($\mathcal{I}^p$) is the contravariant form of the other.

Einstein laid down the principle that the equations which describe any physical process must satisfy the condition that their covariance with respect to arbitrary substitutions can be deduced from the invariance of ds . In other words, the laws of nature must be represented by equations which are covariant for the form

$$\sum_{p,q=0}^3 g_{pq} dx^p dx^q$$

with respect to all point-transformations of co-ordinates. Laws of nature are assertions of *coincidences* in space-time, and, therefore, must be expressible by covariant equations.

It might be thought that by following up the consequences of this principle we should obtain important positive results. However, Ricci and Levi-Civita had shown long before that from practically *any* assumed law we can derive another law which does not differ from it in any way that can be tested by observation, but which is covariant. The fact that a formula has the covariant property does not, therefore, tell us anything as to whether it is correct or not. We are, however, perhaps justified in believing that a conjectural law which can be expressed readily and simply in covariant form is more worthy of attention (as being more likely to be true) than one whose covariant form is awkward and complicated.

Not only must the general laws of physics be covariant, it is also necessary that every single assertion which has a physical meaning must be covariant with respect to arbitrary transformations of the co-ordinate system. Thus the assertion that an electron is at rest for an interval of time of duration unity cannot have a physical meaning, since this assertion is not covariant.

In Einstein's general theory, the velocity of light at any place has always the value c *with respect to any inertial frame of reference* for this neighbourhood, and the velocity of any material body is less than c . Thus there is no difficulty in the fact that the fixed stars have velocities greater than c with respect to axes fixed in the rotating earth: for such axes are not inertial.

Some physicists called attention to the fact that when light is propagated in a medium where there is anomalous dispersion, the index of refraction may be less than unity, whence it seemed as if the velocity of light in the dispersive medium might be greater than the velocity of light *in vacuo*. The difficulty was removed when it was pointed out by L. Brillouin and A. Sommerfeld that the velocity of light with which the index of refraction is concerned is the *phase* velocity, whereas the velocity of a signal is the *group* velocity, which is never greater than c .

It has sometimes been supposed, by a misunderstanding, that the general Einsteinian theory requires us to regard the Copernican conception of the universe as no more true than the Ptolemaic, and that it is indifferent whether we regard the Earth as rotating on her axis or regard the stellar universe as performing a complete revolution about the earth every twenty-four hours. The root of the matter, by which everything is explained, is that the Copernican axes are inertial, while the Ptolemaic are not. The earth rotates *with respect to the local inertial axes*.

In his paper (51), published in 1913, Einstein (at p. 241) gave the form which Maxwell's equations of the electromagnetic field must take in a gravitational field, i.e. when the matrix of space-time is given by a quadratic differential form

$$(ds)^2 = \sum_{p,q=0}^3 g_{pq} dx^p dx^q.$$

If $\mathbf{d}$ is the electric vector and $\mathbf{h}$ is the magnetic vector, then we know that

Maxwell's equations in Euclidean spaces consist of the Ampère–Maxwell tetrad

$$\begin{aligned}\frac{\partial d_x}{\partial x} + \frac{\partial d_y}{\partial y} + \frac{\partial d_z}{\partial z} &= 4\pi\rho, \\ \frac{\partial h_z}{\partial y} - \frac{\partial h_y}{\partial z} &= \frac{1}{c} \frac{\partial d_x}{\partial t} + 4\pi\rho v_x, \\ \frac{\partial h_x}{\partial z} - \frac{\partial h_z}{\partial x} &= \frac{1}{c} \frac{\partial d_y}{\partial t} + 4\pi\rho v_y, \\ \frac{\partial h_y}{\partial x} - \frac{\partial h_x}{\partial y} &= \frac{1}{c} \frac{\partial d_z}{\partial t} + 4\pi\rho v_z,\end{aligned}$$

and the Faraday tetrad

$$\begin{aligned}\frac{\partial h_x}{\partial x} + \frac{\partial h_y}{\partial y} + \frac{\partial h_z}{\partial z} &= 0, \\ \frac{\partial d_z}{\partial y} - \frac{\partial d_y}{\partial z} &= -\frac{1}{c} \frac{\partial h_x}{\partial t}, \\ \frac{\partial d_x}{\partial z} - \frac{\partial d_z}{\partial x} &= -\frac{1}{c} \frac{\partial h_y}{\partial t}, \\ \frac{\partial d_y}{\partial x} - \frac{\partial d_x}{\partial y} &= -\frac{1}{c} \frac{\partial h_z}{\partial t}.\end{aligned}$$

Now write $x^0 = ct$, $x^1 = x$, $x^2 = y$, $x^3 = z$:

then the electric and magnetic vectors together constitute a six-vector

$$d_x = X^{01}, d_y = X^{02}, d_z = X^{03}, h_x = X^{23}, h_y = X^{31}, h_z = X^{12},$$

and the Ampère–Maxwell tetrad of equations written in covariant form are

$$\Delta iv (X^{pq}) = 4\pi \mathcal{F}^p, \quad (\text{A})$$

where $\Delta iv (X^{pq})$ denotes the *vectorial divergence* of X^{pq} , that is

$$\sum_{q=0}^3 (X^{pq})_q,$$

where the suffix outside the bracket denotes covariant differentiation, and where $\mathcal{F}^p$ denotes the four-vector which represents the electric charge and current; and the Faraday tetrad written in covariant form is

$$\Delta iv (Y^{pq}) = 0, \quad (\text{B})$$

where Y_{pq} is the six-vector dual to X_{pq} , i.e. such that

$$Y_{pq} = \sqrt{(-g)} X^{rs},$$

where (p, q, r, s) is an even permutation of the numbers $(0, 1, 2, 3)$. The

equations (A) and (B) represent the equations of the electromagnetic field in space-time of any metric whatever.

The six-vector of the electromagnetic field may be expressed in terms of potentials, in the same way as in Euclidean space it is expressed by the equations

$$d_x = -\frac{\partial \phi}{\partial x} - \frac{1}{c} \frac{\partial a_x}{\partial t}, \quad h_x = \frac{\partial a_z}{\partial y} - \frac{\partial a_y}{\partial z},$$

etc. For if we write $(\phi_0, \phi_1, \phi_2, \phi_3)$ for $(\phi, -a_x, -a_y, -a_z)$, these equations become

$$X_{pq} = (\phi_p)_q - (\phi_q)_p,$$

where the suffixes outside the brackets represent covariant differentiation: *and we see that the potential $(\phi_0, \phi_1, \phi_2, \phi_3)$ is a covariant vector.*

Electromagnetic theory lends naturally to physical optics, and this again to geometrical optics. Now in the relativity theory of Poincaré and Lorentz, for which the line-element $d\tau$ in the world of space-time is given by

$$(d\tau)^2 = (dt)^2 - \{(dx)^2 + (dy)^2 + (dz)^2\}/c^2$$

the geodesics of the world are straight lines, and the null geodesics (i.e. the geodesics for which $d\tau$ vanishes) are the straight lines for which

$$\frac{(dx)^2 + (dy)^2 + (dz)^2}{(dt)^2} = c^2;$$

so the null geodesics are the tracks of rays of light. When Einstein created his new general theory of relativity, in which gravitation was taken into account, he carried over this principle, and asserted its truth for gravitational fields. The principle was, however, not proved at that time: and indeed there was the obvious difficulty in proving it, that strictly speaking there are no 'rays' of light—that is to say, electromagnetic disturbances which are filiform, or drawn out like a thread—except in the limit when the frequency of the light is infinitely great; in all other cases diffraction causes the 'ray' to spread out.

The matter was investigated in 1920 by M. von Laue, who, starting from the partial differential equations for electromagnetic phenomena in a gravitational field, obtained a particular solution which corresponded to light of infinitely high frequency, and showed that the path of this disturbance satisfied the differential equations of the null geodesics: thus for the first time proving the truth of Einstein's assertion. It was afterwards shown by E. T. Whittaker that the law is really an immediate deduction from the theory of the characteristics of partial differential equations, and that it is not necessary to introduce the notion of frequency at all: in fact, that *in a gravitational field, any electromagnetic disturbance which is filiform must necessarily have the form of a null geodesic of space-time.*

In September 1913 Einstein lectured on 'gravitation' to the Physics Section of the *Naturforschenversammlung* at its fifth meeting, held in Vienna. From

the discussion that followed, it was clear that many German men of science were not yet converted to his ideas. Doubts were expressed regarding the validity of his views on the equality of inertial and gravitational mass, on the velocity of propagation of gravitational processes, on the possibility of ever being able to detect the deflexion of light rays in a gravitational field, and on the predicted red-shift of spectral lines in such a field. Dr R. W. Lawson, who was present, recalls that the older generation was more sceptical than the younger men, some of whom later became distinguished workers in special and general relativity.

In 1913 it was decided to invite Einstein to settle in Berlin as a member of the Royal Prussian Academy of Sciences in connexion with the Kaiser Wilhelm Gesellschaft, which had recently been founded by Wilhelm II as a centre of research institutes. Max Planck and Walther Nernst, who were at the time the leading German physicists, journeyed to Zurich to convince Einstein of the desirability of the plan, and persuaded him to accept it; he left Zurich for Berlin at the end of 1913.

Not long after making the change he separated from his wife Mileva Maric. He now became attached to another lady, his cousin, Elsa, whom he had known as a child in Munich. She was the daughter of a business man, and was now a widow with two daughters. The marriage to Mileva was dissolved on 14 February 1919, although in 1924 she formally obtained leave to continue to use the name *Einstein*. She and Einstein's two sons continued to live in Switzerland. Mileva never left Switzerland, and died in Zurich on 4 August 1948; the elder son became a civil engineer, and eventually settled in America. He is now professor of hydraulic engineering at the University of California, Berkeley. The younger son had a nervous breakdown at about the age of 20, from which he never recovered. Elsa was married to Einstein and lived with him as his wife until her death in 1936.

In the papers that have been referred to, which are of date earlier than November 1915, Einstein gave a satisfactory account of the behaviour of mechanical and electrical systems in a field of gravitation which is supposed given: his formulae were derived fundamentally from the principle of equivalence, i.e. the principle that the systems behave just as if there was no gravitational field, but they were referred to a co-ordinate system with an acceleration equal and opposite to the acceleration of gravity. But he had not as yet succeeded in obtaining an entirely satisfactory set of fundamental equations for the gravitational field itself, i.e. equations which would play the same part in his theory that Poisson's equation,

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} = -4\pi\rho,$$

played in the Newtonian theory. This defect was repaired, and the theory (now known as *General Relativity*) substantially completed, in a series of short papers, (68), (69), (70), published in November–December 1915, in the *Berlin Sitzungsberichte*.

Let us first inquire what expressions covariant with respect to the form

$$\sum_{p,q} g_{pq} dx^p dx^q$$

can be formed from the g_{pq} and their derivatives alone. It can be shown that these can all be derived from a certain tensor of rank 4, known as the *Riemann tensor*, which is denoted by K_{pqrs} . From the Riemann tensor we can obtain a tensor of rank 2 which is defined by the equation

$$K_{pq} = \sum_{r,s} g^{rs} K_{pqrs};$$

it is called the *Ricci tensor* or *contracted-curvature tensor* and from the Ricci tensor we obtain what is called the *scalar curvature* of the space, defined by the equation

$$K = \sum_{p,q} g^{pq} K_{pq}.$$

For a two-dimensional space, e.g. a surface in Euclidean three-dimensional space, $-\frac{1}{2}K$ is the ordinary Gaussian measure of curvature $1/\rho_1\rho_2$.

Now Mach had long before introduced a principle, that inertia must be reducible to the interaction of bodies; Einstein generalized this into what he called *Mach's principle*, namely that the field represented by the ten potentials g_{pq} is determined *solely* by the masses of bodies. The word 'mass' is here to be understood in the sense given to it by the theory of relativity, that is, as equivalent to energy. Now energy is expressed covariantly by an *energy-tensor* of rank 2, introduced in 1908 by Hermann Minkowski, which is denoted by T_{pq} : so in the fundamental equations of gravitation, corresponding to the equation $\nabla^2 V = -4\pi\beta\rho$ of the Newtonian theory (where β is the Newtonian constant of gravitation), we may expect the tensor T_{pq} or some linear function of it, to take the place of Poisson's ρ . We expect to find on the other side of the equation, corresponding to $\nabla^2 V$, a tensor of the same rank as T_{pq} , that is, the second rank, containing second derivations of the potentials, but no higher derivatives. The only covariant tensors of this character are the Ricci tensor K_{pq} , with K_{pq} and g_{pq} . Einstein first supposed that K_{pq} might be a simple constant multiple of T_{pq} : but this is not satisfactory, since the divergence of T_{pq} is zero and the divergence of K_{pq} is not in general zero: and he finally proposed the equations

$$K_{pq} = -\kappa(T_{pq} - \frac{1}{2}g_{pq}T) \quad (p, q = 0, 1, 2, 3),$$

where

$$T = \sum_{p,q} g^{pq} T_{pq}$$

and κ is a constant depending on the Newtonian constant of gravitation. *These are the general field-equations of gravitation.*

Multiplying them by g^{pq} , summing with respect to p and q , and remembering that

$$\sum_{p,q} g^{pq} g_{pq} = 4$$

we have $K = \kappa T$, so the equations may be written

$$K_{pq} - \frac{1}{2} g_{pq} K = -\kappa T_{pq} \quad (p, q = 0, 1, 2, 3).$$

These are ten equations for the ten unknowns g_{pq} : there are four identities between them, as might be expected, for four of the g_{pq} can be assigned arbitrarily as functions of the x^p , corresponding to the fact that the equations are invariant under the most general transformation of co-ordinates.

In the complete form of the theory as developed later, material bodies are regarded as singularities in the field, i.e. regions where the gravitational field-equations $K_{pq} = 0$ are not satisfied; and eventually he proved that the 'equations of motion' which determine the motion of these singularities in presence of each other can be deduced from the field equations.

According to Mach's principle as adopted by Einstein, the curvature of space is governed by physical phenomena, and we have to ask whether the metric of space-time may not be determined *wholly* by the masses and energy present in the universe, so that space-time cannot exist at all except in so far as it is due to the existence of matter. The point at issue may be illustrated by the following concrete problem: if all matter were annihilated except one particle which is to be used as a test-body, would this particle have inertia or not? The view of Mach and Einstein was that it would not; and in support of this view it may be urged that, according to the deductions of general relativity, the inertia of a body is increased when it is in the neighbourhood of other large masses: it seems needless, therefore, to postulate other sources of inertia, and simplest to suppose that *all* inertia is due to the presence of other masses. When we confront this hypothesis with the facts of observation, however, it seems that the masses of whose existence we know—the solar system, stars, and nebulae—are insufficient to confer on terrestrial bodies the inertia that they actually possess; and, therefore, if Mach's principle were adopted, it would be necessary to postulate the existence of enormous quantities of matter in the universe which have not been detected by astronomical observation, and which are called into being simply in order to account for inertia in other bodies. This is, after all, no better than regarding some part of inertia as intrinsic.

The relation of the constants in Einstein's and Newton's laws of motion is readily found: the constant κ of the Einsteinian theory is connected with the Newtonian constant, the β of Poisson's equation $\nabla^2 V = -4\pi\beta\rho$, by the equation

$$\kappa = \frac{8\pi\beta}{c^2}.$$

In (69), the third of his papers in the *Berlin Sitzungsberichte* of 1915,

Einstein showed that the new gravitational theory could explain an anomaly that had long been known to affect the motion of the perihelion of the planet Mercury, namely that the line of apsides advances $43''$ in a century. It was shown much later by H. R. Morgan* that the earth's perihelion also has a secular motion, much smaller in amount, which agrees with the amount calculated by Einstein's theory. Other explanations of these perihelion effects are, however, not altogether impossible.

A second comparison of General Relativity with observation proposed by Einstein, was that a ray of light coming from a star and passing close to the sun's gravitational field, when observed by a terrestrial observer, should be deflected through an angle of about $1.75''$. This prediction was tested at the solar eclipse of May 1919, and was at the time regarded as confirmed observationally; but later eclipses gave somewhat different results, and the matter must be regarded as still unsettled.

A third observational test proposed was the displacement to the red of spectral lines emitted in a strong gravitational field: here, however, complications are introduced by other possible factors. On the whole question of the comparison of General Relativity with observation, as it was regarded in the early days cf. E. Wiechert, *Ann. Phys., Lpz.* **63**, 301-381 (1920).

The philosophy of science exhibited in Einstein's work had some novel and characteristic features. He rejected the view of the British empiricists, that knowledge in physics is obtained straightforwardly by applying the methods of abstraction and induction to facts ascertained by experience: and on the other hand he rejected the teaching of Kant, that there are certain self-evident truths, such as the axioms of geometry, which are derived from reason alone, independently of experience. The Einsteinian doctrine is that a mathematician, contemplating Nature, can find systems of mathematical equations which satisfy certain conditions and are worthy of trial as furnishing possibly correct physical theories. One such condition is invariance: the meaning of the equations is not to be affected by any arbitrary change in the choice of co-ordinates. Another condition is simplicity: he had (like Kepler) a mystical belief that the simplest equations are the most likely to be true; and yet another condition is that in special cases they should reduce to results already known to be true in those cases. Systems of equations of immense generality can in this way be found and afterwards confronted with the facts of observation.

Philosophically, the work of Einstein has obviously much in common with Descartes's doctrine of vortices; in both cases the theory was entirely a mental construction proposed for examination as a possible explanation of experience; and doubtless much of the opposition that Einstein encountered was due to a survival of the disfavour into which cartesianism had by this time fallen. Some of it may have been due to the popular principle attributed to Rutherford, that *an alleged scientific discovery has no merit unless it can be explained to a barmaid*. In 1916 Einstein published (75) and (82), a new and extremely

* *Astrophys. J.* **51**, 127 (1945).

simple proof of Planck's law of radiation, and at the same time obtained some important results regarding the emission and absorption of light by molecules. The train of thought followed was more or less similar to that adopted by Wien in the derivation of his law of radiation, but Einstein now adapted it to the new situation created by Bohr's theory of spectra.

Consider a molecule of a definite kind, disregarding its orientation and translational motion: according to quantum theory, it can take only a discrete set of states $\mathcal{Z}_1, \mathcal{Z}_2, \dots, \mathcal{Z}_n, \dots$, whose internal energies may be denoted by $\epsilon_1, \epsilon_2, \dots, \epsilon_n, \dots$. If molecules of this kind belong to a gas at temperature T , then the relative frequency W_n of the state $\mathcal{Z}_n$ is given by the formula of Gibbs's canonical distribution as modified for discrete states, namely (for simplicity omitting consideration of the statistical 'weight' of the state)

$$W_n = e^{-\epsilon_n/kT}.$$

Now suppose that the probability of a single molecule in the state $\mathcal{Z}_m$ passing in time dt spontaneously, i.e. without excitation by external agencies (as in the emission of γ -rays by radioactive bodies) to the state of lower energy $\mathcal{Z}_n$ with emission of radiant energy $\epsilon_m - \epsilon_n$ is

$$A_m^n dt \tag{A}$$

Suppose also that the probability of a molecule under the influence of radiation of frequency ν and energy-density ρ passing in time dt from the state of lower energy $\mathcal{Z}_n$ to the state of higher energy $\mathcal{Z}_m$ by absorbing the radiant energy $\epsilon_m - \epsilon_n$, is

$$B_n^m \rho dt \tag{B}$$

and suppose that the probability of a molecule under the influence of this radiation-field passing in time dt from the state of higher energy $\mathcal{Z}_m$ to the state of lower energy, $\mathcal{Z}_n$, with emission of the radiant energy $\epsilon_m - \epsilon_n$, is

$$B_m^n \rho dt \tag{B'}$$

This is called *stimulated emission*; its existence was recognized here for the first time.

Now the exchanges of energy between radiation and molecules must not disturb the canonical distribution of states as given above. So in unit time, on the average, as many elementary processes of type (B) must take place as of types (A) and (B') together. We must therefore have

$$e^{-\epsilon_n/kt} B_n^m \rho = e^{-\epsilon_m/kt} (B_m^n \rho + A_m^n)$$

We assume that ρ increases to infinity with T , so this equation gives

$$B_n^m = B_m^n \tag{1}$$

and the preceding equation may, therefore, be written

$$\rho = \frac{(A_m^n/B_m^n)}{\exp\{(\epsilon_m - \epsilon_n)/kT\} - 1}.$$

This is evidently Planck's law of radiation: in order that it may pass asymptotically into Rayleigh's law for long wavelengths, and into Wien's law for short wavelengths, we must have

$$\epsilon_m - \epsilon_n = h\nu$$

and

$$A_m^n = \frac{8\pi h\nu^3}{c^3} B_m^n. \quad (2)$$

The two equations (1) and (2), first given in this paper of Einstein's, are fundamental in the theory of the exchanges of energy between matter and radiation, and have been extensively used in the later development of quantum theory.* The formulae were expanded to the case of non-sharp energy-levels by R. Becker,† and to the laws of interaction between radiation and free electrons by Einstein & Ehrenfest (122).

Another important result established in this paper related to exchanges of momentum between molecules and radiation. Einstein showed that *when a molecule, in making a transition from the state $\mathcal{Z}_n$ to $\mathcal{Z}_m$, receives the energy $\epsilon_m - \epsilon_n$, it receives also momentum of amount $(\epsilon_m - \epsilon_n)/c$ in a definite direction: and, moreover that when a molecule, in the transition from $\mathcal{Z}_m$ to the state of lower energy $\mathcal{Z}_n$, emits radiant energy of amount $(\epsilon_m - \epsilon_n)$, it acquires momentum of amount $(\epsilon_m - \epsilon_n)/c$ in the opposite direction.* Thus the processes of emission and absorption are *directed* processes; there seems to be no emission or absorption of spherical waves.

In 1916 and the following years Einstein gave much attention, specially (78), (91), (172), to the propagation of disturbances in a gravitational field. If the distribution of matter in space is changed, e.g. by the circular motion of a plate in its own plane, gravitational waves are generated which are propagated outwards with the speed of light. If such waves impinge on an electron which is at rest, the principle of equivalence shows that the physical situation is the same as if the electron were moving with a certain acceleration, and therefore *an electron exposed to gravitational waves must radiate.*

In 1917 Einstein pointed out, (83), that the field equations of gravitation, as he had given them in 1915, do not satisfy Mach's principle, according to which no space-time could exist except in so far as it is due to the existence of matter (or energy). Einstein's equations of 1915, however, admit the particular solution

$$g_{pq} = \text{constant}, \quad T_{pq} = 0, \quad (p, q = 0, 1, 2, 3),$$

so that a field is thinkable without any energy to generate it. He, therefore, proposed now to modify the equations by writing them

$$K_{pq} - \frac{1}{2}g_{pq}K - \lambda g_{pq} = -\kappa T_{pq} \quad (p, q = 0, 1, 2, 3)$$

The effect of the λ term is to add to the ordinary gravitational attraction

* If the weights of the energy-levels are g_n, g_m , the relation (1) must be replaced by $g_n B_n^m = g_m B_m^n$. Relation (2) is unaffected.

† *Z. Phys.* **27**, 173 (1924).

between particles a small repulsion from the origin varying directly as the distance; at very great distances this repulsion will no longer be small, but will be sufficient to balance the attraction; and in fact, as Einstein showed, it is possible to have a statical universe spherical in the spatial co-ordinates with the uniform distribution of matter in exact equilibrium. This is generally called the *Einstein universe*.* The departure from Euclidian metric is measured by the radius of curvature R_0 of the spherical space, and this is connected with the total mass M of the particles constituting the universe by the equation

$$\frac{\gamma M}{c^2} = \frac{1}{4}\pi R_0,$$

where γ denotes the Newtonian constant of gravitation. The total volume of this universe is $2\pi^2 R_0^3$. The cosmological problem when the λ -term is not introduced was discussed in 1931 in (157).

After 1918 there were many exhibitions of feeling in Germany hostile to Einstein, led by the eminent physicist Philipp Lenard of Heidelberg, who was an early member of Hitler's party. The Prussian Minister of Education, Hänisch, wrote to Einstein begging him to take no notice of these attacks, with such effect that Einstein, who in Imperial Germany had retained his Swiss nationality, now became a German citizen.

The development of relativity after 1918 was greatly influenced by a theory put forward in that year by Weyl,† in which the attempt was made to represent electromagnetic forces, as gravity was represented, as consequences of the pure geometry of space and time. Weyl's geometry, instead of being specified like Einstein's by a single quadratic differential form

$$\sum_{p,q} g_{pq} dx^p dx^q$$

was specified by a quadratic differential form

$$\sum_{p,q} g_{pq} dx^p dx^q$$

and a linear differential form

$$\sum_p \phi_p dx^p$$

together. The coefficients g_{pq} of the quadratic form can be interpreted as in Einstein's theory as the potentials of gravitation, while the four coefficients ϕ_p of the linear form can be interpreted as the four components of the electromagnetic potential-vector. Thus Weyl succeeded in exhibiting both gravitation and electricity as effects of the metric of the world. Einstein wrote on this theory in (106) (1921), demurring to the physical validity of some of Weyl's considerations.

* The suggestion that our universe might be an Einsteinian space-time of constant *spatial* curvature seems to have been first made by Ehrenfest in a conversation with De Sitter about the end of 1916.

† *S.B. preuss. Akad. Wiss.* part I, p. 465 (1918); *Math. Z.* **2**, 384.

The enlargement of geometrical ideas thus achieved by Weyl was followed in 1921 by a still wider extension due to Eddington,* which was based on an analysis of the notion of parallelism in curved space, due to T. Levi-Civita.† In 1923 Eddington's theory was further developed (118), (119), (120), by Einstein. The primitive quantities in this theory, in terms of which the space-time manifold and its properties are described, are not the fourteen metrical and electrical tensor-components g_{pq} , ϕ_r , but forty quantities $\Gamma_{\mu\nu}^a$ which specify the parallel-transport and are called *components of the affine field*. A contracted Riemann tensor of the second rank is built up from these quantities in the usual way, and is then split into symmetric and skew parts which are identified respectively with the gravitational g_{pq} and the electrical ϕ_r . In these papers Einstein obtained the forty equations which determine the $\Gamma_{\mu\nu}^a$ as functions of the g_{pq} and the ϕ_r from an Action principle $\delta \int \mathbf{H} dt = 0$, where $\mathbf{H}$ is a scalar density and the variation is with respect to the $\Gamma_{\mu\nu}^a$ as independent variables.

Weyl's proposal for a unified theory of gravitation and electromagnetism was followed up in another direction by Th. Kaluza,‡ in whose system the ten gravitational potentials g_{pq} of Einstein and the four components ϕ_r of the electromagnetic potential were expressed in terms of the line-element in a space of five dimensions, in such a way that the equations of motion of electrified particles in an electromagnetic field became the equations of geodesics. This theory was discussed approvingly by Einstein in 1927 (138). In 1921 Einstein was invited by Chaim Weizmann, the leader of the Zionist movement, who was a lecturer in organic chemistry in the University of Manchester, to accompany him on a journey to the United States, where they were received with tremendous enthusiasm. In returning, he visited England.

In 1924 Satyandra Nath Bose of Dacca University gave a new derivation of Planck's formula of radiation, based on the assumption that radiation is composed of photons, which for statistical purposes can be treated like the particles of a gas, but with the important difference that photons are indistinguishable from each other, so that instead of considering the allocation of individual distinguishable photons among a set of states, he fixed attention on the number of states that contain a given number of photons. Bose's paper showed that photons obey a particular kind of statistics, different from the classical statistics of Maxwell, and that the recognition of this fact leads immediately to Planck's law of radiation. Einstein seems to have translated this paper into German from an English manuscript sent to him by Bose, and immediately extended it, (125) and (131), to the study of a monatomic ideal gas; a photon (apart from its polarization property) differs from a monatomic molecule only in that its static mass is vanishingly small. The average number

*. *Proc. Roy. Soc. A*, **99**, 104 (1921).

† *R.C. Circ. mat. Palermo*, **42**, 173.

‡ *S.B. preuss. Akad. Wiss.* (1921) p. 966.

of particles of mass m in unit volume of such a gas, with energies in the range ϵ to $\epsilon + d\epsilon$, is

$$\frac{2\pi}{h^3} \frac{(2m)^{\frac{3}{2}} \epsilon^{\frac{1}{2}} d\epsilon}{e^{(\epsilon/kT) + \mu} - 1}$$

where μ is a constant. This is the fundamental formula of what is generally called *Bose-Einstein statistics*.

In 1928 Einstein introduced, (140), (141), (145), (146), (151), (152), (153), (154), (158), a proposal for a unitary theory of the gravitational and electromagnetic fields, by devising a Riemannian geometry with new invariants and tensors which retained the notion of parallelism between line-elements at a distance from each other, as in Euclidean geometry.* In (159) (1931) the concept of parallelism at a distance was dispensed with. In (160) (1932) he discussed the line-element of space-time applicable to the universe as a whole, when it is assumed that at every instant the spatial section is flat and the cosmical constant λ is zero, showing how, from observations of the velocity of recession of the nebulae, the mean density of matter in the universe might be estimated.

For the winter of 1932 the Einsteins went to the California Institute of Technology at Pasadena. In January 1933 Adolf Hitler was appointed Chancellor of the German Reich and the 'purge' of Jews began. Einstein went back to Europe, but not to Germany; in the spring of 1933 he settled in a Belgian town near Ostend and wrote to Planck resigning his position in Berlin. In October 1933 he sailed from Southampton for New York, to accept a position in the Institute for Advanced Study, being appointed a member of the Institute for life. Later he acquired American citizenship. At Princeton he published joint papers with many disciples.

A unified field theory somewhat different from that of 1928, in which the fundamental tensor was taken to be Hermitian, was developed in 1945-8, (185), (187), (189); and yet another, announced in the press at Christmas 1949, was published in 1950 (192): this made use of a non-Riemannian geometry based on a fundamental tensor g_{pq} , which is analyzed into a symmetric and a skew part: and was further modified in (194), (195).

The appeal of General Relativity has in recent years been weakened by a growing doubt as to whether continuous differential equations in four-dimensional space-time can possibly provide a solution of some of the problems of quantum theory, such as those relating to the time of decay of a radium atom. This has been the subject of many papers. Einstein took one side, holding that the world can be completely described by a field theory of the type of General Relativity, which is rigidly deterministic; cf. (167), (169). Most other theoretical physicists, particularly Max Born, Niels Bohr, and Wolfgang Pauli, maintain on the contrary that in micro-physics strict causality must be abandoned. The matter is discussed by many writers in

* Accounts of this theory are given by Eddington in *Nature, Lond.* **123**, 280-1 (23 February 1929), and by Einstein in *Ann. Inst. Poincaré*, **1**, 1-24 (1931).

the volume *Albert Einstein: Philosopher-Scientist*, edited by P. A. Schilpp. The last paper which had Einstein as its sole author (196), was devoted to this question of the interpretation of quantum-mechanics.

Einstein died on 18 April 1955. For much help in the preparation of this notice I am indebted to his son, Professor Hans Albert Einstein, and to Professor Dr E. Stiefel of Zürich. The photograph is by Karsh of Ottawa.

EDMUND WHITTAKER

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Enrico Fermi

ENRICO FERMI

1901-1954

THE sudden death of ENRICO FERMI at the age of 53 has filled physicists all the world over with greatest sadness and consternation. One of the most outstanding and in some respects unique scientific personalities, a wonderful teacher and a marvellous representative of his native country, Italy, has left us.

Fermi was born in Rome on 29 September 1901. He was educated at the High School in Rome and later at the Scuola Normale Superiore of Pisa where he obtained a Doctorate in 1922. He later studied at Göttingen with Born and at Leiden with Ehrenfest. From 1924-26 he was Lecturer in Mathematical Physics at Florence. In 1927 he was elected to a Professorship of Theoretical Physics in Rome and in 1929 became one of the Founder Members of the Royal Academy of Italy.

Fermi's early work was mostly concerned with theory, often with problems which arose from the advent of the new mechanics of Heisenberg, Dirac and Schrödinger. One group of investigations dealt with spectroscopy: the anomaly of the intensity ratio of the multiplets of the higher alkali metals, the magnetic moments of nuclei, calculations of spectra of ions, the Raman effect in CO_2 and in crystals, the oscillations and rotations of the NH_3 molecule and the hyperfine structure separation.

Into the same period, when he was in Rome, falls his theory of a gas whose particles obey Pauli's exclusion principle. A preliminary study of the conditions under which degeneration of a gas can take place (18)* led him to the correct partition function at about the same time (21) as Dirac developed his theory of an ideal gas. The method of Fermi's derivation is anything but elegant, being based on an oscillator model, but it has the advantage of showing up what he was doing. This clarity is one of the reasons why Fermi's papers appeal to the experimental physicists (1). Fermi rapidly recognized the general usefulness of the statistical method and applied it with conspicuous success to the calculation of various atomic properties. This Fermi-Thomas method was later applied by F. Bloch with success to calculate the stopping power of matter for charged particles. Here again one strength of Fermi's becomes apparent: the skill and instinct with which he obtained approximate results where accurate calculation would be prohibitively complex.

Though Fermi was not a theoretical formalist, he was greatly interested in

* Numbers in parentheses refer to the numbered entries in the bibliography.

and contributed to the development of Dirac's radiation theory and to quantum electrodynamics at about the same time as Heisenberg and Pauli performed their work on a similar topic. An exposition of this topic of unsurpassable clarity was published by Fermi in the *Review of Modern Physics* of January 1932.

Not unconnected with his interest in field theory was his paper on 'An attempt at a theory of beta rays'. Here Pauli's *ad hoc* explanation of the continuous beta-ray spectrum by the neutrino is put into a theory, formally constructed in a manner similar to that of the emission of electro-magnetic radiation. There is a certain arbitrariness in the choice of the interaction Hamiltonian. Fermi apologizes for the particular choice by saying it was the simplest. At the time, his theory was objected to, among other reasons, on the grounds of being at variance with the experiments, particularly in so far as the low energy spectrum of the electrons was concerned. Very characteristically, Fermi pointed out that the experiments were difficult and perhaps their precision not sufficient to be decisive to test his theory! Only shortly before, N. Bohr, in his Faraday Lecture (1932), had stressed the great dilemma of physics in so far as beta decay was concerned. Though Pauli's proposal of a neutrino was the lesser of two evils (the other meant giving up conservation of energy in the beta decay) and was a relief to all, Fermi's theory reproduced a considerable number of experimental facts and made the neutrino hypothesis more plausible to our minds (*International Conference on Physics* Vol. 1, p. 67 (1934)). Today Fermi's theory is essentially accepted over a wide range, though nature has turned out to be more complex than we thought.

Both his work on quantum statistics and on the theory of the beta decay had by this time (1934) established Fermi's international fame as a theoretical physicist.

In 1933 Fermi's activity entered a new stage, when after Joliot-Curie's discovery of the production of artificial radioactivity by α -particle bombardment he took up neutron research as an experimenter or a theoretician as the situation required. By that time he had collected round him a number of young Italian physicists all of whom were to obtain international repute in later life. Fermi learnt to construct Geiger counters and was able to use the radon from the 1 gram of radium belonging to the Bureau of Public Health to build a radon-beryllium neutron source. With this he began a systematic search through the elements for artificial radioactivity, starting with hydrogen. He obtained no positive results for the elements up to and including oxygen but found that fluorine was strongly activated. In this work he collaborated with Amaldi, D'Agostino, Pontecorvo, Segré and Rasetti.

The neutron source and the counter were kept at the end of a long corridor to prevent interference with measurements. Mrs Fermi has described how the short-lived elements required fast running to take them from the source to detector and Fermi and Amaldi excelled in this. Of the 63 elements investigated 37 were shown to have an easily detectable activity. The

activation cross-section was found not to depend in any systematic way on the atomic nucleus. Two types of transmutation were discovered leading to emission of protons or α -particles and a third in which the neutron was captured with emission of a gamma ray. The heavy elements were usually transmuted by neutron capture and led to a single unstable element with a single exponential decay of radioactivity. Uranium and thorium proved, however, to be exceptional for several radio-active elements were produced. In a letter to *Nature* of 16 June 1934, Fermi described the existence of activities having half-lives of 10 seconds, 40 seconds, 13 minutes and at least two longer period activities. The 13-minute activity was shown not to be isotopic with elements 82, 83, or 88-92. He therefore suggested tentatively that a transuranium nucleus with charge 93 might have been formed. It was not till the discovery of uranium fission by Hahn and Strassman $3\frac{1}{2}$ years later that the reason for the complex radioactivities was discovered. One of the activities of 2.3 days half life was shown by E. McMillan in early 1940 to be due to element 93—neptunium.

During the course of the experiments in 1934, Amaldi and Pontecorvo found that the intensity of the radiocativity induced by neutrons varied with the surroundings of the specimen. The radioactivity of silver was found to be increased 100 times by surrounding it with paraffin. Water produced a similar effect and it was inferred that this was due to the slowing down of the neutrons increasing the activation cross-section—in some cases, 1000 times. These neutrons were shown to make 100 collisions in paraffin before capture. This work was the precursor of the atomic pile. On the advice of Corbino a joint patent for the production of artificial radioactivity by slow neutrons was taken out and was the subject of an award in the United States after the war. The main results appeared in two papers (56) and (57) communicated by Lord Rutherford on 25 July 1934 and 15 February 1935 for publication in the *Proceedings of the Royal Society*. Paper I is mostly concerned with the activities induced by unmoderated sources; II contains a large assortment of observations such as efficiency of various slowing down materials, scattering and diffusion of the slow neutrons, temperature effects, the large variation of capture cross sections for different elements, emission of gamma rays on neutron capture, separation of radioactive isotopes, and a list of all activities found. A section on theoretical considerations on the properties of slow neutrons yields much of the picture of the neutron capture process, such as the $1/v$ law. Fermi was quite aware of the theoretical difficulties caused by the existence of finite capture cross sections for fast neutrons, a fact which was only later elucidated by Bohr's postulate of the compound nucleus formation. The theory of the slowing down of neutrons, which was later to become so important in theoretical calculations on atomic piles, is already contained in a simple form in a memorandum published by the Consiglio Nazionale delle Ricerche, Roma, 1934, under the title 'Sul Moto dei neutroni nelle sostante idrogenate'. Here it is also pointed out that the large n - p low energy scattering is mainly due to the singlet state of the deuteron and that

the neutron-capture cross section of hydrogen is due to a magnetic dipole transition. With this one simple assumption of Fermi's many incomprehensible features of the deuteron system obtained a natural explanation. As the physics of the deuteron system is of fundamental importance to nuclear physics, just as the hydrogen atom to atomic physics, any contribution in such a field is of particularly high merit.

In the years following, i.e. 1936-7, about ten papers appeared mostly in the *Ricerca Scientifica* on the diffusion and absorption of neutrons, work mostly carried out in collaboration with Amaldi. The last note in this is dated July, 1937.

In the autumn of 1938 the anti-semitic movement in Italy, which developed after the Italian-German alliance, made Fermi decide to leave Italy to take up a professorship at Columbia University. He was awarded the Nobel Prize in December 1938 and the journey to Stockholm helped his move to the United States.

At Columbia Fermi joined forces with H. L. Anderson, Zinn and Szilard, to study the possibility of developing a chain reaction in uranium. About the same time as in France it was shown that neutrons were emitted in the fission process. Uranium was then surrounded with water to moderate the neutron energy, but it was found that ordinary water absorbed too many neutrons to make a chain reaction possible. Szilard and Fermi then decided to try a pile of graphite blocks interposed with lumps of uranium metal. When I (J.D.C.). visited Columbia in November 1940, I saw Fermi carrying out these experiments—at the same time as Halban and Kowarski were carrying out experiments on a heavy water nuclear chain reaction in Cambridge.

By the spring of 1941 a small pile had been built at Columbia, but was too small to become divergent. At the end of 1941 the group went to Chicago and Anderson and Zinn built a larger pile in the squash court of the University of Chicago. Of this period Anderson has said . . . 'Fermi possessed a sure way of starting off in the right direction, of setting aside the irrelevancies, of seizing all the essentials and proceeding to the core of the matter. The whole process of wresting from nature her secrets was for Fermi an exciting sport which he entered into with supreme confidence and great zest. No task was too menial if it sped him towards his goal. He thoroughly enjoyed the whole of the enterprise. The piling of the graphite bricks, the running with the short lived activated rhodium foils, and the merry clicking of the Geiger counter which effected the measurement. All was done with great energy and obvious pleasure, but by the end of the day, in accordance with his plan, the results were neatly compiled, their significance assessed, and the progress measured, so that early in the morning on the following day, the next step could begin.

It was a feature of the Fermi approach never to waste time—to keep things as simple as possible, never to construct more elaborately or to measure with more care than was required by the task at hand. In such matters his judgement was unerring. In this way, step by step, the work sped forward until in less than four short years Fermi had reached his goal. A huge pile of graphite

and uranium had arisen in the West stands of the University of Chicago Campus. When, on 2 December 1942, 12 years ago, Enrico Fermi stood before that silent monster he was its acknowledged master. Whatever he commanded it obeyed. When he called for it to come alive and pour forth its neutrons it responded with remarkable alacrity; and when at his command it quieted down again, it had become clear to all who watched that Fermi had indeed unlocked the door to the Atomic Age.'

In 1943 Fermi and his family went to Los Alamos to join Oppenheimer on the atomic bomb project.

One of us (E.B.) had the following impressions of this period:

'During part of my stay at Los Alamos I was in charge of the experimental group of the division for advanced development, headed by Fermi. It is natural, therefore, that we had a good deal of contact with Fermi who took great interest in what we were doing. An incident which is very characteristic of Fermi occurred when some particularly important and surprising results were obtained. When I told Fermi about it, he said, "Please give me the experimental data and I will calculate the final result and if my calculations agree with you then probably the results obtained are correct." It was very characteristic of Fermi that he would not accept an experimental result but try to find out in detail how it was obtained. He was always available for any discussion and when he reported about the work of the division he was always very generous and fair in allotting credit where it was due.

'Besides the profound influence of his research work, I believe that he had very considerable influence on American physics through his lecturing and teaching at the University of Chicago. He was able, practically without preparation, to present any topic in nuclear and atomic physics with clarity and restriction to the essentials, which permitted nearly everybody to follow. This often happens with good lecturers, but frequently one finds that after one has left the lecture hall most of what one thought one understood has vanished. In Fermi's case the attraction of his exposition rested in the fact that one really understood the problem and did not, therefore, have to rely on memory. He was quite free from oratory but simply had a very penetrating insight into a problem and could formulate his thoughts clearly. The same quality is found in most of his papers; one of them, with Marshall, practically started off the whole interaction of neutrons with solids. Everything is there, just to be exploited.

'During my last summer at Los Alamos, when Fermi came for a visit, I asked him what he was doing. He said: "Well, I have one big course where I teach children and, you know, I find it much more difficult than giving a highbrow course." I said to Fermi: "I am really jealous of your students: I wish someone like you would teach me physics under the same circumstances." The next day he came to my office and said: "I am quite prepared to give you a small private course provided you find half a dozen suitable people to listen." I gave him a list of a dozen people: he crossed half of them off—those he thought would not be suitable—and then started on a course of

radiation theory and quantum electrodynamics. It was a very great experience and extremely profitable for us. The only trouble was that Fermi did not seem to get tired and instead of one hour he lectured for two; we were too exhausted, but it was obvious that he could easily have gone on for a further period. He just knew the topic so well, in spite of its difficulties, that it cost him no effort to reproduce it.

'As a person, Fermi seemed simplicity itself. He was extraordinarily vigorous and loved games and sport. On such occasions his ambitious nature became apparent. He played tennis with considerable ferocity and when climbing mountains acted rather as a guide. One might have called him a benevolent dictator. I remember once at the top of a mountain Fermi got up and said: "Well, it is two minutes to two, let's all leave at two o'clock"; and of course, everybody got up faithfully and obediently. This leadership and self-assurance gave Fermi the name of "The Pope" whose pronouncements were infallible in physics. He once said: "I can calculate anything in physics within a factor 2 on a few sheets: to get the numerical factor in front of the formula right may well take a physicist a year to calculate, but I am not interested in that." His leadership could go so far that it was a danger to the independence of the person working with him. I recollect once, at a party at his house when my wife cut the bread, Fermi came along and said he had a different philosophy on bread-cutting and took the knife out of my wife's hand and proceeded with the job because he was convinced that his own method was superior. But all this did not offend at all, but rather charmed everybody into liking Fermi. He had very few interests outside physics and when he once heard me play on Teller's piano he confessed that his interest in music was restricted to simple tunes.'

After the war Fermi naturally made full use of the availability of high neutron fluxes from the Argonne reactors. Again the happy ability to turn to experiments or attack a problem theoretically bore fruit lavishly. The bare statement of titles of his papers is impressive: 'Production of low energy neutrons by filtering through graphite' (72), 'Transmission of slow neutrons through microcrystalline materials' (74); 'Interference phenomena of slow neutrons' (76). 'Phase of scattering of thermal neutrons by aluminium and strontium' (75); 'Spin dependence of scattering of slow neutrons by Be, Al, Bi' (79); 'by deuterium' (82); 'On the interaction between neutrons and electrons' (81); 'A thermal neutron velocity selector and its application to the measurement of the cross section of boron' (78). In this large amount of work he was essentially only supported by Leona Marshall, though in some cases other collaborators helped, such as W. J. Sturm, R. G. Sachs, H. L. Anderson. Some papers are astonishingly comprehensive; the one on interference phenomena starts with the theory introducing the scattering lengths, the basic convenient datum easily extracted from experiments, Bragg reflexions, filtered neutrons, diffraction by gaseous molecules, total reflexion by mirrors, concluding with a good table of scattering lengths deduced from his results. Only the cream of neutron optics had been skimmed off but it was

rich, and much in the direction of refinement could be done and was achieved at the Argonne, Oak Ridge and Brookhaven Laboratories. But Fermi's basic work, together with his teaching of the subject at the University of Chicago, gave immense impetus to the development in this field. The effect of this genial atmosphere and the high standard set by Fermi can still be felt in many places remote from Chicago.

When the cyclotron began to operate Fermi participated in the work of the cyclotron group. The scattering of pions with protons and deuterons was studied in detail and great efforts were made to obtain the phase shifts. Fermi's theoretical work this time was concerned with a statistical, strong interaction model for the multiple production of pions at very high particle energies (85) suitable to evaluate the observations of the Brookhaven cosmotron.

In parallel with the above research, Fermi had taken a considerable interest in W. A. Hiltner's observation of the polarization of star light and its interpretation by L. Davis and J. L. Greenstein as magnetic dichroism of interstellar material caused by weak magnetic fields of huge extension. He showed that wandering magnetic fields can, on the average, accelerate particles to cosmic ray energies provided their initial energy is above a certain limit. It seems at present that Fermi's basic idea is correct and that a considerable step forward has been made in a field which has been extremely puzzling. It is worth noting that Fermi and Chandrasekhar were able to confirm by two independent estimates the strength of field in the spiral arms of our galaxy at 7.2×10^{-6} and 6×10^{-6} gauss.

E. BRETSCHER

J. D. COCKCROFT

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J. T. Hewitt

JOHN THEODORE HEWITT

1868-1954

JOHN THEODORE HEWITT was born at Windsor on 12 October 1868, the son of John Hewitt (1840-1874) and Alice Mary Hewitt (*née* Pasmore). The father had a coachbuilding business, which received most of the work from the Castle. He had previously been apprenticed to a hatter and hosier at Tonbridge and met his wife at meetings of the Plymouth Brethren in Windsor. John Hewitt had three children. The eldest died in infancy. John Theodore was the second and Alice Marian the third. John Hewitt was a studious man, who read his New Testament in Greek. He was, however, a victim of tuberculosis and died at the early age of 34. Shortly afterwards his widow purchased the good-will of a small private school for girls and little boys at Newbury. This was a success. She found time to attend classes in chemistry and electricity and magnetism given in Newbury by a German (titular Professor) Hoffert and took John Theodore, then eleven years old, with her. The boy's interest was aroused and he began to do experiments at home.

In 1881 Mrs Hewitt was appointed Headmistress of the Girls' College and High School at Southampton and succeeded in persuading T. W. Shore, the Principal of the Hartley Institute, to admit her young son, John Theodore, who as a result received instruction in mathematics, classics, machine drawing and chemistry. One of his fellow students was W. R. Bower (later Head of the Physics Department at Huddersfield Technical College) and another was F. W. Lanchester, later of aerodynamic and motor-car fame. The chemistry teaching was in the hands of James Brierley, the Public Analyst for Southampton. At this period Hewitt and Bower became walking enthusiasts: they apparently thought nothing of a day's walk of thirty or forty miles. Walking remained one of Hewitt's favourite pastimes.

In 1884, Bower and Hewitt won National Scholarships and started work at the Normal School of Science (later called the Royal College of Science) on 1 October. It may be noted that Hewitt was not sixteen until 12 October. From then until June 1885 Hewitt lived with an aunt and he recorded the fact that one of his cousins, George Andrew Parkinson (1861-1944) was chemist to John Knight & Sons at Silvertown and possessed the recently published *Commercial organic analysis* (Allen) which he (J.T.H.) 'found very instructive'.

Hewitt worked in P. F. Frankland's laboratory, the assistants being Chapman Jones and W. J. Cousins. The most prominent feature seems to have been inorganic qualitative analysis. Hewitt recorded that before he was

allowed to do his first gravimetric estimation (of copper in copper sulphate) he had to analyze qualitatively a mixture containing no less than eighteen different ions. Later in the session he also studied mechanics.

During the session 1885-6 Hewitt lived with his grandmother at Windsor. From October 1885 to February 1886 he studied physics and then until June 1886 geology under Judd. Towards the end of the session there was a short course on astronomical physics. During this period Hewitt got to know H. G. Wells and Richard (later Sir Richard) Gregory.

This year's work had decided Hewitt in favour of chemistry rather than engineering, to which he had at one time inclined. In October 1886 he went back into Percy Frankland's laboratory and attended T. E. Thorpe's first year inorganic lectures, which were extensively illustrated with experiments. In the second half of the session Japp lectured on organic chemistry and Hewitt said that he was thereby delighted. He found it disappointing that the laboratory work was mostly 'inorganic quantitative analysis, gas analysis and vapour densities'. He did a few organic preparations as a piece of extra work. However, he enjoyed this session very much, living now just outside St Denys Station in Southampton. He had to get out of bed at 6 a.m. and catch the 6.58 a.m. train which reached Vauxhall at 9.38 a.m. He usually managed to reach the lecture room at 10 a.m. No one was admitted after five past ten, so that this time-table was a little exacting. On one occasion the train was late and so was Hewitt. He started work in the laboratory, but Frankland noticed the irregularity and said 'See that it does not happen again'. The laboratories closed at 4 p.m. and Hewitt got back home at about 7 p.m. The up and down train journeys gave Hewitt 4 to 4½ hours reading (or the opportunity therefor), but he made a number of friends not only among his fellow-passengers, but also with certain engine drivers. As a result he sometimes went from Basingstoke to Shawford on the foot-plate.

In December 1886 Hewitt sat for scholarships at St John's College, Cambridge and 'got on the foundation'. In January 1887 he passed London Matriculation and in June 1887 the Associateship of the Normal School of Science. As a result of mixing on the train with non-academic men much his senior he wanted 'to take up a business career and not go to Cambridge'. However here he found himself in October 1887. He managed to scrape through Little-go and began to take lectures by James Dewar and Pattison Muir on physical chemistry, although these were meant for Part II of the Tripos! He did some practical chemistry in the St John's laboratory, attended J. J. Thomson's physics lectures and took J. E. Marr's Part II geology lectures. In 1888 he took general biology and from October 1888-May 1889 did botany. In 1889 he passed Part I in chemistry, physics, geology and botany.

In 1887 Hewitt got to know, as his junior, R. A. Lehfeldt, who later became Professor of Physics at East London College and died in 1927 as Principal of the Witwatersrand University. He also got to know Alfred Mond, F. F. Blackman and F. S. Locke, with whom he played poker. He was not interested

in games, but joined the University Volunteers and relied on walking for exercise.

In July 1889 Hewitt took the London Intermediate Science Examination and on the strength of this secured a chemistry exhibition, a physics exhibition and a Neil Arnott medal.

Having passed Part I, Hewitt settled down to do Part II in chemistry, with physics as 'subsidiary'. He worked under Siegfried Ruhemann in the then new University Chemical Laboratories designed by the Professor of Chemistry, G. D. Liveing. The two demonstrators were W. J. Sell and H. J. H. Fenton. James Dewar had part of the laboratory at his disposal. According to Hewitt, Ruhemann's lectures 'were a delight. His enthusiasm over a piece of work such as the sugars or uric acid was immense. He almost ran from one end of a long blackboard to the other writing structural formulae at an incredible speed'. Ruhemann was assiduous in his laboratory supervision.

In 1890 Hewitt got a First in Part II of the Tripos and after a Continental holiday returned to Cambridge and, at the suggestion of Ruhemann, began an investigation on 'chlorinated phenylhydrazines': he prepared *o*- and *p*-chlorophenylhydrazine and made a few simple derivatives. He did some private coaching and later became a junior demonstrator. In October 1890 he obtained a First Class in the London degree and was once more awarded a scholarship on merit. He was thus able to take Ruhemann's advice and work for a time in Germany. He started in Heidelberg in April 1891. Victor Meyer was the Professor, von Auwers, Gattermann, Paul Jacobson and Jannasch being a.-o.-professors. Hewitt began under Gattermann on the interaction of quinalizarin (1:2:5:8-tetrahydroxyanthraquinone) and ammonia. He took no lectures, but put in long hours in the laboratory. One evening in the week there was a meeting of the Heidelberg Chemische Gesellschaft at which Professors and students turned up. 'The chemical part of the evening came first and was accompanied with beer; at the end of serious matters, cigars and pipes were lit.' At Whitsuntide there was a week's holiday, which Hewitt spent with a friend on one of his beloved walking tours.

In October 1891 Hewitt went back to Cambridge and did some demonstrating, while continuing his researches on hydrazines and beginning an investigation of citraconfluorescein, the condensation product of resorcinol with citraconic anhydride. At the end of the May Term Hewitt went to Berlin and worked on citraconfluorescein in the old laboratory in Georgenstrasse, where Tiemann had just succeeded Hofmann. Hewitt returned to Cambridge in October, did demonstrating and coaching and some Extension lecturing and in December 1892 went to Heidelberg. He took the examination for Ph.D. in January 1893. He only got 'cum laude', because he 'did botany as his third subject although he was better at mathematics'. In June 1893 Hewitt took the London D.Sc., one of the examiners being H. E. Armstrong, who set the candidate two essays (1) on the evidence for 4-membered rings and (2) on the symmetry of anthraquinone.

In the summer of 1893 Hewitt worked for some weeks in the Hartley

Institute Laboratory at Southampton and discovered the first of the aryl-azophenol hemihydrates. He went back to Cambridge in October 1893 and sat for a Fellowship. He did not get one, but later on said he was very glad he didn't, 'as it would have meant staying in Cambridge'.

Having seen a 'Professorship' of chemistry at the People's Palace Technical Schools advertised he applied for it and was elected. He started work there at the end of the Lent Term, 1894. There was a Day School and an Evening School. David Allan Low, the well-known engineer, was Headmaster of the Day School and John Leigh Smeathman Hatton, later Principal of East London College, was Director of Studies for evening students. Here Hewitt found F. G. Pope, who was a demonstrator. Hewitt's 'hours' were twelve per week in the Day School and three hours each on Monday, Tuesday, Wednesday and Friday evening. On Saturday there were no classes, but Hewitt and Pope usually went to the laboratory at about 3 p.m. and worked until 9.45 p.m. Hewitt recorded with enthusiasm the help he received at this period from his research students H. A. Phillips, A. E. Pitt and T. S. Moore and from J. J. Fox. Hewitt lectured on pure chemistry and also on coal tar products. Two of his (evening) students were two of the Crow brothers, who owned a tar distillery at Barking. Clarence Smith, later Editor of the *Journal of the Chemical Society*, joined the staff in 1903 and because he and Pope were very much organic chemists Hewitt had to do the physical.

In 1910 Hewitt was elected a Fellow of the Royal Society, and in 1915 East London College, as it was then called, became a School of the University and Hewitt became a University Professor. When the College, in 1934, was renamed Queen Mary College, Hewitt was made one of the first Fellows.

In some autobiographical notes Hewitt mentioned four of his East London colleagues as having been his particular friends—J. T. Morris (later McGregor-Morris), Professor of Electrical Engineering, C. H. Lees (Professor of Physics), W. H. White, Lecturer in Physics and T. C. Hodson, the Registrar. Outside the College Hewitt mentioned H. E. Armstrong and his family, W. J. Pope, Arthur and Kathleen Lapworth, Julian L. Baker, M. O. Forster, K. J. P. Orton and Raphael Meldola.

Just before 1900 Hewitt had become friendly with F. W. Tunnicliffe, who was doing medicine at St Bartholomew's Hospital and was a favourite pupil of Lauder Brunton. The idea that not alcohol but the other components of spirits were the cause of any ill effects led Hewitt and Tunnicliffe to visit a distillery in Scotland. It was thought that furfural was the offending substance and to remove it Hewitt tried adding the calcium salt of phenylhydrazine-*p*-sulphonic acid. The result was equal to expectations and Hewitt and some friends formed a Company for maturing spirits. The process was tried out at one of Dewar's whisky distilleries, but did not appeal to that firm because too much of the flavour was taken out of the whisky. An Irish distillery in Tullamore did, however, use the maturing process for many years.

Hewitt said that 'the direct pecuniary results were negligible' but he got a reputation as an expert in spirits. He was called in on two law cases—for the

prosecution on 'What is brandy?' and for the distillers on 'What is whisky?'.

The biggest legal case in which Hewitt was involved was that between the Bayer Company and the English importers. He was associated with H. E. Armstrong and P. F. Frankland, whilst Dewar was on the other side to advise Fletcher Moulton and other prominent patent lawyers. (Bayer's application for a patent for the manufacture of acetylsalicylic acid had been refused in Germany though granted in England.) As a result of the law case the patent was declared bad and had to be annulled. This was said to have been a heavy financial loss to Bayers.

The 1914-1918 war came as no surprise to Hewitt, who knew Germany from many angles. During 1914-1915, with C. H. Lumsden, he worked out methods of making acetic anhydride, which was not manufactured in Great Britain. The method finally chosen made use of sodium acetate and sulphur mono-chloride. Early in 1915 Hewitt suggested to G. D. Lander (of the Royal Veterinary College) that the much desired toluene should be obtainable from mixtures of benzene and xylene in presence of AlCl_3 (the Jacobsen reaction). They worked this successfully first on the laboratory scale and Messrs F. W. Berk & Co. of West Ham arranged to adapt this to a semi-works scale.

At this juncture Hewitt received a commission as 'Major, General List, attached Royal Engineers' and with H. R. Le Sueur had quickly to acquire a uniform and start for the Dardanelles.

Hewitt did a large number of water analyses and among other things was called in to advise about a cargo of chlorine cylinders. In January 1916 he was sent home and after a short leave was seconded to the Department of Explosives Supply (Ministry of Munitions), under Lord Moulton. He admired Moulton, who was a hard worker expecting his subordinates to work. At first Hewitt was associated with R. C. Farmer at Storey's Gate, but later was put in charge of an ex-German laboratory at Chiswick. Among the staff he had here were Clarence Smith, W. S. Denham, H. S. Patterson and W. J. Jones. The work was at first on explosives and then on chemical warfare materials and involved much visiting of factories in different parts of the country. Hewitt enjoyed it all very much, partly because it enabled him to see things done on the large scale.

During this period Hewitt lived with his mother and his sister, for both of whom he had deep affection and admiration. These three seem to have been real companions with different but wide interests. Mrs Hewitt died in December 1918.

At the beginning of 1919 Hewitt resigned from his professorship and the 'S.H.M.' Co. Ltd was formed, the six shareholders being T. D. Morson, C. S. Roy, Clarence Smith, J. L. Baker, Miss Hewitt and J. T. Hewitt. Smith and Hewitt were joint managing directors, with Sutton Manor House, Heston, Middlesex, Hewitt's latest home, as their headquarters. Miss Hewitt seems to have had a pretty time-filling allocation in the way of duties. The firm started with making quinine ethyl carbonate, which necessitated first

preparing ethyl chloroformate from alcohol and phosgene. Other products made were quinoline, methylquinolines, benzthiazoles, phenylhydrazine, and (with ethyl acetoacetate not of their making) phenylmethylpyrazolone. In 1924 the firm broke up and Hewitt did odd examining and consulting work. In the early '30's he served on two interesting Departmental Committees, one on fungicides and insecticides and the other on the effect of the quality of glass on the maturation of wine in bottles.

In 1928 Hewitt and his sister left Heston and went to Rose Cottage, Lines Road, Hurst, near Twyford. The cottage was 'old and rather dilapidated'. Hewitt had to have two sheds built, to provide a sitting room and a balance room. There were no gas, electricity or water mains and no main drainage. Here Hewitt, now sixty, did some technical work, wrote several dictionary articles and 'read a considerable amount of mathematics and physics with considerable pleasure and little understanding'.

Hewitt's sister, of whom he was so fond, died early in 1943 and thereafter he lived alone in the cottage with three cats. He still went on reading in chemistry in the broadest sense. He had little interest in literature, art or music except in a general kind of way. He had always been indifferent to games and he disliked sports of the hunting type. He liked railway travelling if it was 'really fast'.

Considering the period in which Hewitt worked, his scientific investigations covered a very varied field.

During his Cambridge days he found that the product of coupling diazotized *o*-chloroaniline and phenol appeared to exist in two forms, yellow m.p. 85° C and bright red m.p. 96° C. This he first explained on the assumption that the two forms either were respectively hydroxyazo and quinone-hydrazone in structure or were *cis* and *trans* forms of the hydroxyazo compound. He tried to obtain two different acetyl derivatives from the two forms, but failed. Repeating this work at the People's Palace Technical Schools Hewitt found that the supposed isomerism was non-existent, the yellow compound being the hemihydrate of the bright red compound.

In 1897, with F. G. Pope, Hewitt found that benzeneazophenol and its *o*-, *m*- and *p*-methyl and chloro-derivatives could be converted into 'hydrochlorides' by passing hydrogen chloride into their solutions in benzene. Treatment of these 'hydrochlorides' with water gave anhydrous azophenols in the case of benzeneazophenol, and *m*- and *p*-chloro- and *p*-methyl-benzeneazophenol, and hemihydrates in the case of *o*-chloro- and *o*-methyl-benzeneazophenol. Hewitt and Pope showed at the same time that benzeneazophenetole, which was known to form a hydrochloride, also formed a crystalline platinichloride $(C_{14}H_{14}N_2O)_2 \cdot H_2PtCl_6$. This work was extended in conjunction with T. S. Moore and A. E. Pitt. Although several arylazophenols were found not to give hemihydrates, only *o*-nitrobenzeneazophenol gave no hydrochloride.

In 1900 Hewitt published the results of his first studies of nitration of hydroxyazo compounds. It was known that the nitration of benzeneazo-

phenol in concentrated sulphuric acid solution gave 4-*p*-nitrobenzeneazophenol, but Hewitt found that with careful control of temperature and with nitric acid sufficiently dilute to preclude salt-formation between the acid and the azophenol 'in its tautomeric condition of quinonephenylhydrazone', the product was 4-benzeneazo-2-nitrophenol.

With W. G. Aston, Hewitt showed that bromination of benzeneazophenol in acetic acid, in presence of sodium acetate to prevent the formation of the hydrobromide, gave as first product 4-benzeneazo-2:6-dibromophenol, whereas in absence of sodium acetate the product was 4-*p*-bromobenzeneazo-2:6-dibromophenol.

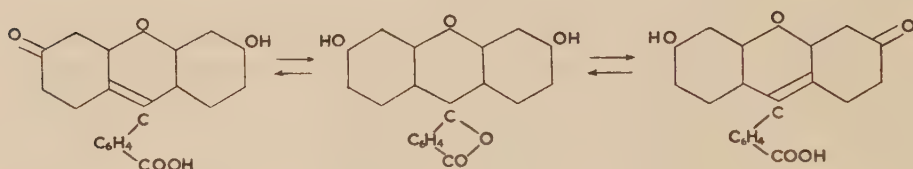
In 1901, Hewitt & H. A. Phillips investigated the bromination of an *o*-hydroxyazo compound. Bromination of benzeneazo-*p*-cresol was shown to occur *ortho* to the hydroxyl group. This was then regarded as a surprising result, since *o*-hydroxyazo compounds were thought to be quinonehydrazones, so that Hewitt & Phillips expected to obtain *p*- (or *o*-) bromobenzeneazo-*p*-cresol, the directing influence being the imino group. Hewitt and Phillips concluded that one of the following must be true: (*a*) that *o*-hydroxyazo compounds, like *p*-hydroxyazo compounds, have the true hydroxyazo structure or (*b*) *o*-hydroxyazo compounds in hydrocarbon solvents behave as quinonehydrazones but in acetic acid solution as hydroxyazo compounds or (*c*) in solution hydroxyazo compounds and quinonehydrazones are in equilibrium, but the hydroxyazo type is so much more reactive than the quinonehydrazone type that only derivatives of the former are obtained.

In an extension of this work, Hewitt & S. J. M. Auld in 1902 found that bromination of benzeneazo- α -naphthol in glacial acetic acid in presence of sodium acetate gave a monobromo compound which when submitted to reductive scission gave aniline, showing that bromination had not occurred in the benzene nucleus—an indication that benzeneazo- α -naphthol was an azophenol and not a quinonehydrazone. Hewitt and Auld also studied the nitration of benzeneazo- β -naphthol. With hot fuming nitric acid the azo-group was displaced and 1:6-dinitro- β -naphthol was formed, whilst with concentrated nitric-sulphuric acid an almost quantitative yield of *p*-nitrobenzeneazo- β -naphthol was obtained. With bromine in glacial acetic acid benzeneazo- β -naphthol gave *p*-bromobenzeneazo- β -naphthol. *o*-Nitrobenzeneazophenol, in which the basic property of the azo-nitrogen was depressed by the *o*-nitro group, was found by Hewitt and H. V. Mitchell (1905) to give the same product, 2':3-dinitro-4-hydroxyazobenzene, whether nitration was carried out in concentrated nitric-sulphuric acid mixtures or in dilute nitric acid. Another interesting observation (Hewitt & N. Walker 1906) was that 3-nitro-4-hydroxyazobenzene, when treated with bromine in glacial acetic acid containing sodium acetate, gave the 5-bromo compound.

Since 1:6-dinitro- β -naphthol is formed in the nitration of β -naphthol, Hewitt & Mitchell thought that this pointed to the possibility of a quinonoid relationship between the 2- and 6-positions. They therefore acted on 1-bromo- β -naphthol in alkaline solution with a *p*-nitrobenzenediazonium salt, expecting

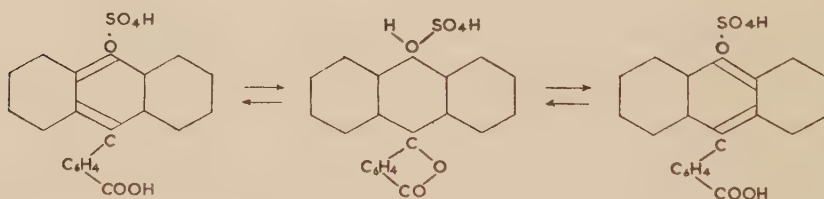
that coupling would be slow and that although the 1-position was blocked, the nitrobenzeneazo group would enter position 6. In fact, coupling was very rapid; the bromine atom was eliminated and the product was 1-*p*-nitrobenzeneazo- β -naphthol. It was concluded at the same time that the known conversion of 1-nitroso- β -naphthol into 1-nitro- β -naphthol by nitric acid represented a replacement, not an oxidation.

In 1900, Hewitt put forward a theory to account for the fluorescence of certain compounds. For example, in the case of fluorescein he suggested that fluorescence was due to oscillation between two equivalent structures, through a transition structure*:

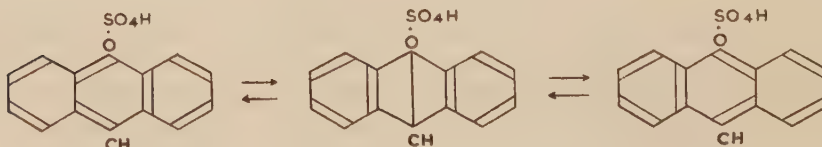


It is interesting to observe how near Hewitt got to the conventional expression of decades yet unborn.

Fluoran shows no fluorescence in alcoholic solution, but gives a strong fluorescence in concentrated sulphuric acid. This was explained as follows:

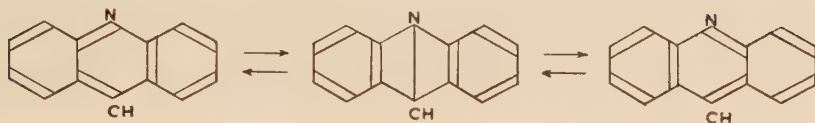


Similarly, xanthidrol is fluorescent in concentrated sulphuric acid solution:

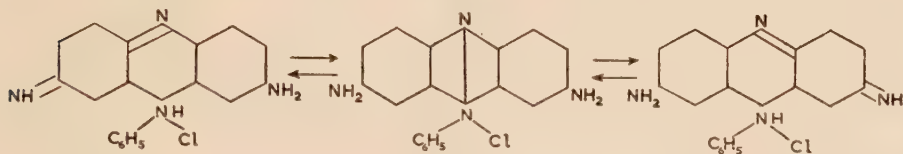


* Aromatic nuclei are written as Hewitt wrote them, sometimes with, but mostly without, the Kekulé double and single bonds. This was essential in order to place the work of Hewitt in its proper perspective.

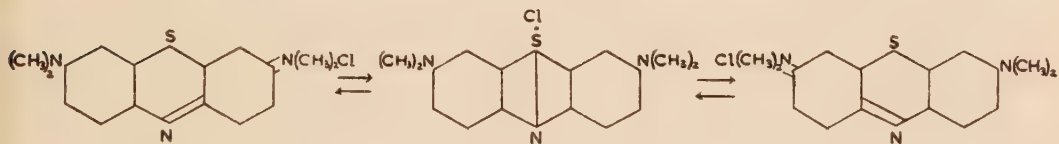
The analogy with acridine is clear:



Phenosafranine shows a yellow-red fluorescence:



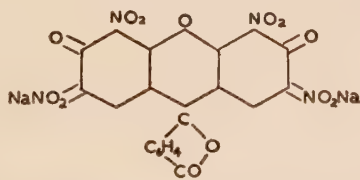
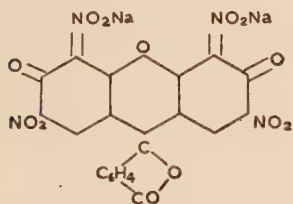
Other examples are: methylene blue:



and anthracene:



Fluorescence was, according to Hewitt, due to 'double symmetrical tautomerism'. The fluorescence of tetrabromo- and tetraiodo-fluorescein fitted in with the theory. In order to explain the non-fluorescence of tetra-nitro-fluorescein in alkaline solution Hewitt & B. W. Perkins (1900) assumed that the sodium salt would be one of the following:



so that opening of the lactone ring and the formation of a paraquinonoid structure was precluded.

Hewitt & A. W. G. Woodforde in 1902 observed that the two isomeric dibromodinitrofluoresceins were non-fluorescent in alkali.

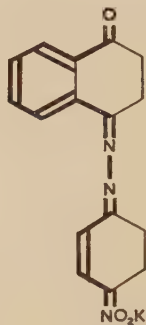
In 1907 Hewitt (with Mitchell) published the first paper on 'Colour and constitution of azo-compounds', in which it was pointed out that the deepening of colour could be attributed to the increase in the length of the conjugation:



5 double; 4 single bonds.



6 double; 5 single bonds.

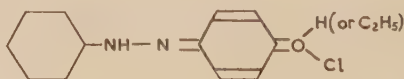


8 double; 7 single bonds.

In the second paper in the same series, Hewitt & J. J. Fox (1908) discussed the constitution of the salts of *p*-hydroxyazo compounds with acids. Farmer & Hantzsch in 1899 had adopted the formula $C_6H_5 \cdot NH \cdot N = C_6H_4 = O$.



When Hewitt (1900) got benzeneazo-*o*-nitrophenol by the action of warm dilute nitric acid on benzeneazophenol, whereas Noetting (1887) had obtained *p*-nitrobenzeneazophenol by nitrating in concentrated sulphuric acid, a radical difference in the constitution of the azophenols and their salts was inferred, the azophenols being regarded as hydroxy compounds, the salts having the Farmer-Hantzsch formula. The ultra-violet absorption spectra of benzeneazophenol and benzeneazophenetole in ethyl alcohol and in concentrated hydrochloric acid (Tuck 1907) were so similar that it was concluded that the hydrochlorides of azophenol have an azo structure. If this is true it is impossible, Hewitt said, to understand the difference in behaviour of hydroxyazo compounds to substituting agents according to whether the free azophenol or a salt with a mineral acid is used: if the hydroxyl group remains as such, substitution must take place first in the hydroxylated nucleus. Fox & Hewitt suggested that the salts of azophenols or azophenetoles with mineral acid should be written:



This fits in with the chemical facts and also, since the salts contain a longer chain of alternate double and single linkings than the parent substances, a shift of the absorption towards the red end of the spectrum is to be anticipated. The nitration of acetanilide and of the sulphate were seen to be comparable:



From these arguments it was evident to Hewitt & W. Thomas (1909) that the change in colour in going from aminoazobenzene to its hydrochloride should be written:



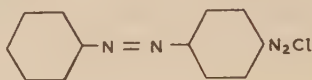
It was thought by the same authors (1909) that *p*-aminobenzeneazophenol, whilst giving yellow solutions in neutral solvents, would give solutions similar to that of methyl orange in presence of a medium amount of mineral acid, but that in concentrated mineral acid the colour would be comparable with that given by benzeneazophenol, the structures of the substance and its mono- and di-acid salts being:



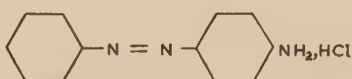
This expectation was fully confirmed by experiment and the probability of the constitution assigned to the respective substances was increased by an examination of alkyl derivatives. The same colour changes were observed when the hydrogen atoms of the NH_2 group alone or of that and the OH group were replaced by methyl. But when *p*-hydroxy- or *p*-methoxy-benzeneazodimethylaniline was converted into a methiodide, while the colour of the salt in neutral solution was yellow, addition of hydrochloric acid produced no crimson shade, although, if the acid were sufficiently concentrated, the orange colour (oxonium salt formation) appeared. Ultra-violet absorption spectra were measured in order to obtain more objective evidence.

An attempt was made by Hewitt & F. B. Thole (1909) to see whether in mineral acid one or two of the benzene rings in benzeneazobenzeneazodimethylaniline, $\text{C}_6\text{H}_5\cdot\text{N}:\text{N}\cdot\text{C}_6\text{H}_4\cdot\text{N}:\text{N}\cdot\text{C}_6\text{H}_4\cdot\text{N}(\text{CH}_3)_2$, could become quinonoid. The point could, however, not be established.

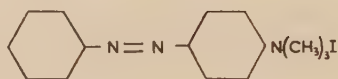
Another ingenious idea of these authors (1910) was to obtain evidence as to the constitution of benzeneazobenzenediazonium chloride



by comparing the absorption spectrum with that of *p*-aminoazobenzene hydrochloride



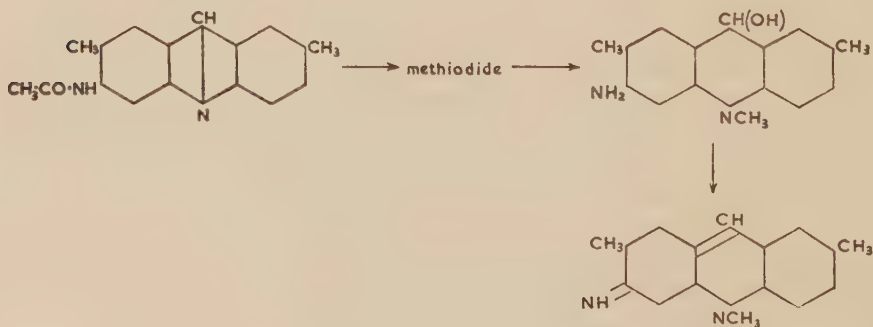
and that of the quaternary salt



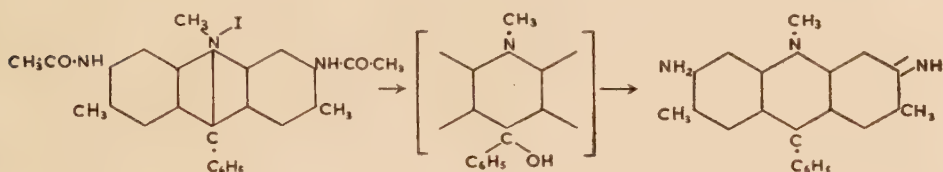
The results, however, were disappointing.

Later, in conjunction with W. Lewcock & F. G. Pope, Hewitt determined the absorption spectrum of stilbene and of its *p*-hydroxy-, *p*-methoxy- and *p*-nitro-derivatives. It was shown that the effect of introducing a nitro-group into stilbene was somewhat similar to that observed in the azo series.

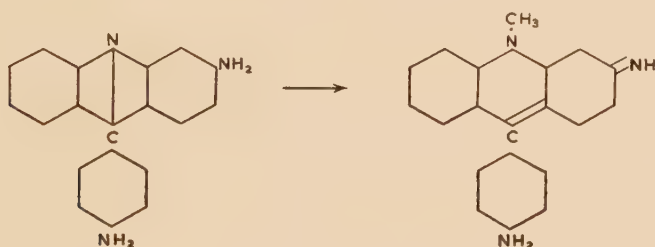
Hewitt was interested not only in the structure and the colour of hydroxy-azo-compounds, but also in acridines and azines and in fact in anything that was coloured or was related to a coloured substance. Incidentally, he was an authority on indicators used in acidimetry. With Fox (1904) he examined the formation of a carbinol base and the relation of the latter to an anhydro-base:



the second stage consisted in heating the methiodide with dilute sulphuric acid and then adding aqueous ammonia. In the benzoflavine series the carbinol base could not be isolated, owing to the ease with which it anhydrides:

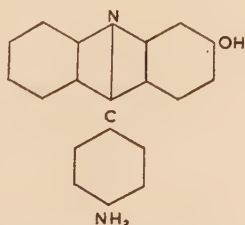


A similar series of changes applied to chrysaniline (2-amino-5-*p*-aminophenylacridine) was used to effect the overall conversion:



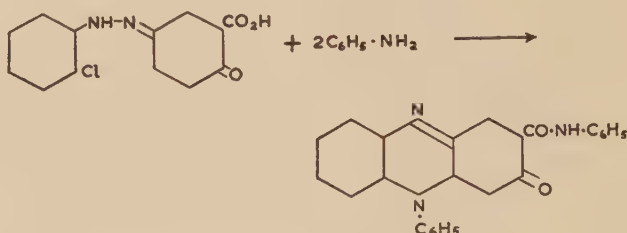
The fluorescence of acridine, 5-phenylacridine and of the NN'-diacetyl-derivatives of benzoflavine and chrysaniline is blue, but the diamino compounds show a green fluorescence in alcohol. In agreement with the Hewitt theory of fluorescence, the more symmetrical benzoflavine shows a much more marked fluorescence than the less symmetrical chrysaniline.

A. E. Dunstan & Hewitt (1908) studied chrysophenol

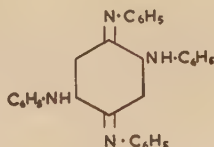


which does not fluoresce in alcoholic solution.

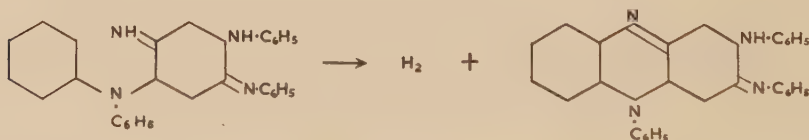
Hewitt's first work on azines was done with H. E. Stevenson, 1896-8. They showed that 2-chlorobenzeneazosalicylic acid (regarded as a quinonehydrazone) was converted by boiling aniline into what was almost certainly an aposafranonecarboxyanilide:



The same authors also investigated azophenine, for which Fischer and Hepp (1888) had proposed the formula,



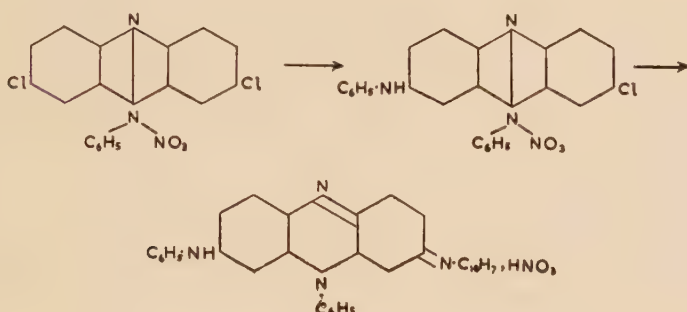
Hewitt & Stevenson thought that the ready conversion of azophenine into phenylinduline was more in accord with a less symmetrical formula for azophenine:



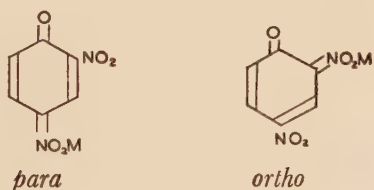
They tested this view. If the symmetrical formula for azophenine is correct, reduction could give only two further secondary amino groups. The unsymmetrical formula would lead to a primary amino group, and the latter was in fact not detectable.

Hewitt returned to the azine series in 1909, when with S. H. Newman & T. F. Winmill he showed that a 'safranol' obtained by Nietzki (1888) by boiling phenosafranine hydrochloride with alcoholic potash was identical with a hydroxy-aposafranone synthesized by Jaubert in 1895 from nitrosophenol and *m*-hydroxydiphenylamine.

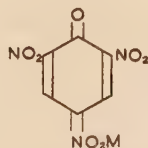
With Kathleen Balls & S. H. Newman, Hewitt in 1912 made some interesting observations on the interaction of dichlorophenylphenazonium nitrate and arylamines. For example, aniline in the cold reacted with one chlorine atom and the product, when heated with α -naphthylamine, gave an anilino-naphthylamino derivative:



Nitrophenols also interested Hewitt. With B. H. Buttler in 1909 he examined the absorption spectra of *o*-nitrophenol, 2:6-dinitrophenol and 2:4-dinitrophenol in neutral and in alkaline solution and concluded that of the two possible quinonoid forms of the metallic salts of 2:4-dinitrophenol, the *para* was preferred:



Similarly picric acid seemed to favour, in its salts, the *para* quinonoid arrangement



In an extension of this work Hewitt at one time (1913) collaborated with Raphael Meldola. With Rhoda M. Johnson and F. G. Pope he drew attention to the non-reactivity of the alkali salts of *o*- and *p*-nitrophenol with halogenoacetic esters as compared with that of simple phenols.

Hewitt's well-informed and lively mind did not confine its attention to a few chemical topics. Anything that a chemist could be interested in interested Hewitt. In 1904, with J. Kenner & H. Silk he made some observations on the bromination of phenol. Three molecules of bromine were known to give tribromophenol, but one molecule or two molecules of bromine gave mixtures in which unchanged phenol and tribromophenol could be found. It occurred to Hewitt that excess of mineral acid might slow down bromination and he and his collaborators found that if two molecules of bromine are added to phenol dissolved in 73 per cent sulphuric acid ($\text{H}_2\text{SO}_4, 2\text{H}_2\text{O}$) over 90 per cent of the product is 2:4-dibromophenol. It was, however, found to be impossible to influence the course of the bromination of α -naphthol by using sulphuric acid. One molecule of bromine, for example, gave 2:4-dibromo- α -naphthol and unchanged α -naphthol.

With T. F. Winmill, in 1907, Hewitt determined the surface tension of a number of phenols, using an ingenious apparatus in which only the vapour of the phenol was present as the non-liquid phase. It was shown that *o*-substituents exerted a steric effect. Hewitt & Winmill (1907) also collaborated in an examination of the arsenic di-iodide described as AsI_2 by Bamberger & Phillipp (1888). The molecular weight was determined and the molecular formula corrected to As_2I_4 . It had been thought that Grignard reagents might react with As_2I_4 to give cacodyl analogues, but phenylmagnesium bromide gave triphenylarsine as the main recognizable product.

Hewitt made a number of contributions to analytical chemistry. In 1908 he had isolated a new basic mercuric chloride, $\text{Hg}_6\text{O}_5\text{Cl}_2$, and found some difficulty in obtaining the analytical equivalent of AgCl because this salt (AgCl) was soluble in mercuric nitrate solution. This led him, with B. H. Buttle, to make a quantitative study of the solubility referred to.

With T. S. Moore in 1902 and W. J. Jones in 1919 Hewitt made noteworthy improvements in the Zeisel method for the determination of methoxyl. With Gladys R. Mann in 1912 he worked out a method for the estimation of ferric iron in the presence of certain organic substances and with the same collaborator made a kinetic study of the reaction between ferric salts and thiosulphates in presence of thiocyanates or phenol or ethyl acetoacetate.

In 1898 Hewitt published an analysis of natural gas obtained during a boring for water at Heathfield Railway Station in Sussex. The composition he found was methane 91.9 per cent, hydrogen 7.2 per cent and nitrogen 0.9 per cent. An analysis made by the County Analyst for East Sussex of gas obtained a month later from the same boring showed a very different composition (oxygen 18 per cent, methane 72.5 per cent, higher hydrocarbons 5.5 per cent and carbon monoxide 4.0 per cent).

During his first few years at East London he wrote an excellent book called *Organic chemical manipulations*. It is strange that this book was allowed to go out of print.

The only academic counterpart of Hewitt's interest in the manufacture of wines and spirits is a piece of work on 'The by-products of alcoholic fermenta-

tion', published with Olive E. Ashdown in 1910. The investigation began in an attempt to discover the source of the acetaldehyde which was formed to some extent in the fermentation of glucose or sucrose. The fermentation of sucrose was caused to take place in presence of constant amounts of potassium phosphate, magnesium sulphate and calcium phosphate, but with varying amounts and sources of nitrogen. It was concluded that formic acid and formamide play a part in alcoholic fermentation.

When he was seventy-seven years of age, Hewitt, in conjunction with T. F. West, took out a patent for a modification of the Skraup synthesis of quinine, in which iodine replaced nitrobenzene. The method is quick and gives good yields.

I first met Hewitt in October 1910 when I entered East London College as a raw undergraduate. As was customary in those days the Professor gave the first year lectures and Hewitt's elementary inorganic lectures were excellent. Any lecture experiment that could be done without elaborate apparatus was done. The phase rule, the lead-chamber process, the atomic weight of hydrogen—all these and others were subjects that Hewitt illuminated and raised above the ordinary by the vigour of his presentation. In the second year, when one might have expected him to lecture on organic chemistry, of which he had so profound a knowledge, he tackled the inorganic and the physical parts of the subject and in the third year became very mathematical for those days when discussing the kinetic theory of gases and similar matters. He occasionally could be induced to give a few special lectures on organic chemistry provided they were not obviously related to probable examination material. I remember some particularly racy lectures on dyestuffs. Nowadays they would doubtless be condemned as containing too much factual knowledge.

Every Friday afternoon Hewitt made his appearance in the laboratory with a record book and visited every student in turn. He looked through the practical note books, made an encouraging comment and entered something in his own book. I remember his congratulating a woman student on her accuracy in determining barium in a *crystalline* specimen of barium chloride. 'Let me see, Miss X, you did dehydrate the salt first, didn't you?'

He liked to get a few students round him in the laboratory and tell them stories of his own student days in Cambridge or Germany. He had, however, no use for idlers and set a high standard because he made one feel that all one did was good fun. His conception of a course in chemistry was something to be done as well and as quickly as possible, so that one could start some research. During his period at East London College one of his students usually topped the list in the Honours Degree common to all London Colleges and those students who did not achieve academic honours at this stage gained from him an impetus that greatly helped them in after life. A high percentage of his students later attained high positions in industry or in the academic world. A few might be mentioned and I deliberately take them at random—

T. S. Moore became Professor in the University of London at Royal Holloway College. When he was a Tutor at Oxford he published with T. F. Winmill a paper which later became famous as the origin of the hydrogen bond. Winmill left University work and later became President of the Anglo-American Tobacco Company. H. A. Phillips was at one time Chief Chemist to the Royal Gunpowder Factory at Waltham. W. G. Hiscock held high positions in the Explosives Industry. A. E. Pitt was later a director of W. J. Bush & Co. R. W. Merriman, who worked with John Wade, was Chief Chemist at Brothertons in Leeds. James Kenner until recently was Professor of Chemistry in the Manchester School of Technology. G. M. Bennett, after being Professor of Chemistry at Sheffield, was Professor at King's College, London, and is now Government Chemist. J. C. Drummond (later Sir Jack) was the first Professor of Biochemistry in London and during the last war was Scientific Adviser to the Ministry of Food. S. J. M. Auld became Professor of Agricultural Chemistry at Reading and is at present the President of the Institute of Petroleum. S. Glasstone is best known for his textbooks and his electrochemical work. A. E. Dunstan and F. B. Thole were lately Chief Chemists to the Anglo-Iranian Oil Company. A. D. Mitchell has been in charge of the Examinations Laboratory of London University and has acted as an Assistant Editor of the *Journal of the Chemical Society* for very many years. J. J. Fox (later Sir John) became Government Chemist. F. G. Pope was a University Reader. R. K. Cannan for many years was a Professor of Biochemistry in New York University and afterwards held high office in Government scientific circles. Five of Hewitt's students became Fellows of the Royal Society.

I was talking about Hewitt to G. M. Bennett recently and we both recalled a vivid incident in 1911, when Hewitt walked quickly down the laboratory, waving a '*Berichte*' and saying: 'Werner has resolved cobalt'. Those were grand days: important qualitative discoveries could still be made and some of them stir the imagination more piquantly than the more cold-blooded quantitative deductions of the mid-twentieth century.

Hewitt had little use for administration, even before it was spelt with a capital A, but he kept up his interest in the chemical affairs of the University of London. An original member of the Board of Studies in 1901, he attended meetings regularly until very recently. If he seldom made a contribution to discussions of University policy, one at any rate felt that if he was willing to listen to a debate it must have some point. In conversation after such meetings it was good to hear his chuckle as he started some reminiscence: 'I remember. . . .'

It is in this mood that those of us who were fortunate enough to know him can safely leave him—a chemist, a gentleman—a gay, kindly figure who could have had no enemies and need never have bothered to count his friends.

E. E. TURNER

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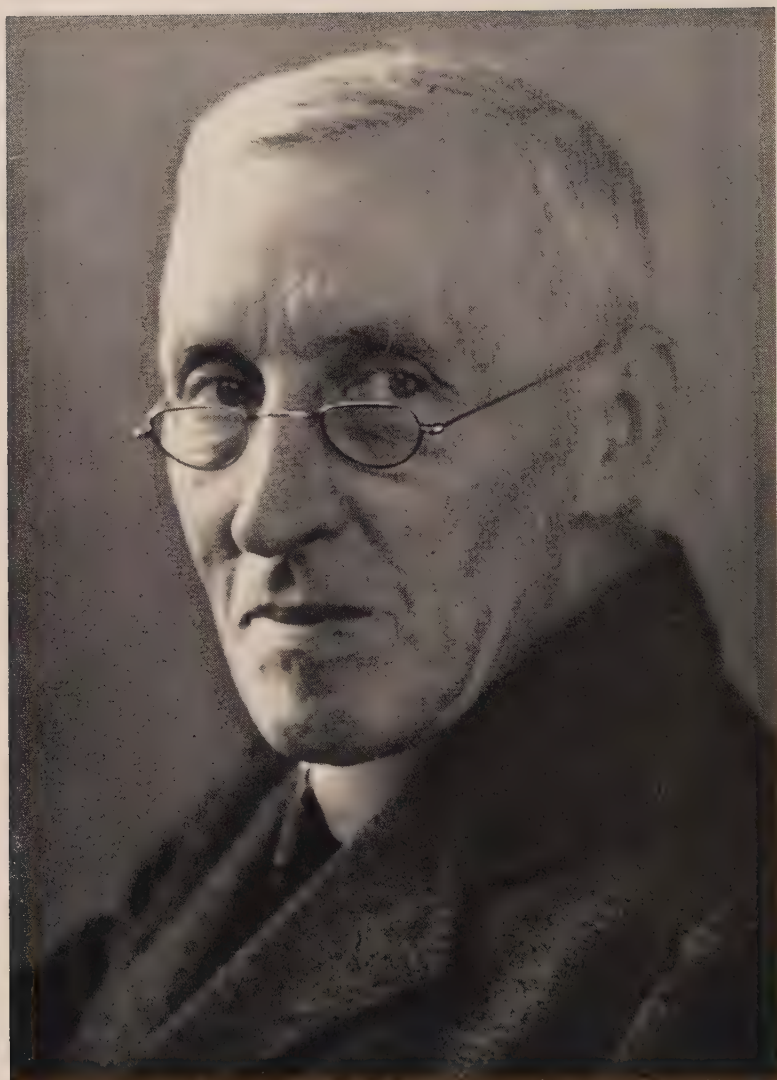
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Joe P. Hill -

JAMES PETER HILL

1873-1954

JAMES PETER HILL was born at Kennoway, Fifeshire on 21 February 1873. He was the younger son of John Hill of Langside, and his wife Catherine Campbell McInroy. Both his parents came from the highlands, his father being born at Scone in Perthshire. John Hill was a farmer, well known as an authority on the breeding of prize cattle—particularly shorthorns for which he won many medals—and as an expert judge of horses.

J. P. Hill went first to the village school at Kennoway where he was chiefly distinguished as a 'grand runner' and he showed his adventurous spirit as a boy of about ten years old by persuading his father to allow him to sail from Leith to Archangel in a small steamer captained by an old family friend. In later years he always told of his trip as 'cabin boy' with great amusement.

Hill then went to the Royal High School, Edinburgh, and, probably because the school did not then provide science teaching, attended classes in biology at the Heriot Watt College during the years 1887-9, gaining 'qualifying teaching certificates' of the Science and Art Department, London, in 'Botany, Vegetable Morphology, Physiology and Animal Behaviour.' At school Hill was a good athlete, playing football for the 1st fifteen.

In 1889 Hill entered the University of Edinburgh as a science student, studying zoology under Professor Cossar Ewart, and botany under Isaac Bayley Balfour. But the man in Edinburgh who had most influence on him was Dr John Beard, University Lecturer in Comparative Embryology and Vertebrate Morphology. Beard was a man of originality who, having worked out in dogfish embryos the place of origin of the primordial germ cells, and their migration into the definitive gonad, went on to develop a theory of the nature of cancer, based on these observations, which has not been substantiated by later work.

It was presumably at Beard's suggestion that Hill, gaining a one-year studentship (later renewed) for teachers in training, migrated in 1890 to the Royal College of Science, London. For the summer session of 1891 he returned to Edinburgh as a junior demonstrator in zoology and botany. At South Kensington Hill was under G. B. Howes, F.R.S., the associate and successor to T. H. Huxley.

Professor F. J. Cole, F.R.S., tells me that Hill was 'one of a large group of young men who were impregnated with a love of animal morphology by the

vast and impressive erudition of G. B. Howes'. Others were Martin Woodward, W. G. Ridewood, R. H. Burne, James Johnstone, and Cole himself. At Howes's house he became friendly with (Sir) Arthur Keith.

In 1892 Professor Haswell of Sydney University wrote to Howes asking him to recommend a young man to demonstrate zoology and botany in his department. Howes recommended J. P. Hill, although he had not graduated or even completed the course. Thus, on 19 August 1892, Hill (aged 19) sailed to Australia, an anniversary he always remembered.

The University to which he came was full of life. Two years before, a new building for the School of Medicine had been completed, the Department of Physiology under Anderson Stuart held in succession the future Sir Alexander MacCormick, Sir Almroth Wright, and Sir Charles Martin as demonstrators, J. T. Wilson had taken charge of anatomy, his newly built department being 'in equipment and facilities for teaching and research far in advance of any comparable department in Great Britain at this time'. The biology department was less active, but Hill immediately set to work, collecting animals of all kinds on land and in the immensely rich harbour and on the shores of Sydney. Thus in five years he published some ten papers, on abnormalities in vertebrates, on Cestodes and Zoantharia, one in which he recorded the first Enteropneust from Australia, and an admirable anatomical account illustrated by magnificent figures of another form from Funafuti.

Early in his time in Sydney Hill came under the influence of J. T. Wilson, then aged 31, who, with a small daughter whose mother had died at her birth, was living with his aged parents. In this household Hill lived until 1898 when, Wilson marrying again, he joined Professor Mills, a surgeon and later Vice Chancellor of Sydney University.

In 1897-8 Hill took leave of absence for six months, returning to Edinburgh to graduate B.Sc., being not unnaturally awarded the Bell-Baxter Scholarship for the best graduate of the year in two science subjects. He had by that time published twelve scientific papers, of which those on an early embryo of *Platypus*, and on the placentation and dentition of *Perameles* are acknowledged classics, marking a revolution in our understanding of mammals.

In 1899, after he had returned to Sydney, Edinburgh awarded to him the 'Gunning' Victoria Jubilee prize for research in zoology, and a George Heriot Research Fellowship of £100 (renewed the following year), and for four years the Government Grant Committee of the Royal Society gave him funds for the study of marsupial and monotreme development. The materials so obtained formed the foundation of much of the later work of Hill and of his school. Small money grants have seldom been used to greater effect.

After his return to Sydney, Hill published his last paper on invertebrates, a small work on zoantharian taxonomy. Then followed in 1899 and 1900 a series of papers on the parturition of *Perameles*, and on the female genital organs of marsupials in general.

On 6 January 1900, at the Presbyterian Church, Finchley, London, J. P.

Hill married Marjorie, eldest daughter of J. Steele, C.B., J.P., Chief Inspector of Inland Revenue, and returned to Sydney, setting up house there.

In 1903 Hill was a candidate for appointment to a newly established Chair of Biology in the Victoria College, Wellington, New Zealand. He submitted testimonials from John Beard, Professor G. B. Howes, Professor Cossar Ewart, Professor A. A. W. Hubrecht, and one from E. Ray Lankester so remarkable that I print it in full here:

‘My dear Dr Hill,

It gives me very great pleasure to add my testimony to that of others in favour of your candidature for the Chair of Biology at Wellington College. In my judgement there is no position in the anatomical and biological world to which you might not aspire. You have alone and by your own original discoveries entirely revolutionized the views of men of science as to the history of the Australian mammals—the marsupials. This you effected by two most careful and interesting investigations—the one on the dentition, the other on the placenta of these animals. I do not know of any man of your age who holds such a record, and I consider that any University should be proud to number you amongst its professors.

With best wishes for your success now and at all times,

I am,

Yours very truly,

E. Ray Lankester,

Director (of the British Museum, Natural History).’

Such a letter from the most distinguished living English zoologist, a man whose judgment of men, as of scientific work, was notoriously critical, is a remarkable testimony to the quality and importance of Hill’s work. But it did not secure his appointment to the Chair.

Hill continued to work in Sydney, now in complete association with J. T. Wilson, on the development of *Ornithorhynchus*, but published nothing for three years. Then in 1906 three Chairs of Zoology fell vacant in England: University College London; the Royal College of Science; and Oxford. Hill was a candidate for each. He was appointed to the Jodrell Chair at University College. Before this appointment was published he was selected for the chair at the Royal College, but refused it having accepted the Jodrell Chair. The Linacre Chair was filled by G. C. Bourne.

Thus in 1906 Hill returned to England and began to reorganize the department he had taken over from Minchin, a specialist on sponges and protozoa. Hill, who in Sydney had cut all his own sections and made his own drawings and photographs, realized that, with the administrative duties he had undertaken, he would not have time to do these technical things, and that the laboratory steward, Mr. H. Jessop (who was originally appointed by Ray

Lankester in the 1870's) was too busy to do much in this direction. Thus he arranged that the College should provide him with a personal technical assistant, who only did ordinary departmental work (mostly photographs) as an exceptional courtesy.

Mr F. J. Pittock, appointed to this post as an untrained lad, was originally taught by Hill and remained with him until Hill's retirement, continuing to work in the Department of Anatomy until his death in 1951. He became extremely skilful, gaining awards from the Royal Photographic Society, and acting as an adviser to a famous firm of platemakers. Thus Hill was the first British zoologist to have the services of a private technician.

From the moment of his appointment Hill devoted himself to teaching, giving four junior lectures a week throughout three terms, taking much more than his full share of senior teaching, including some demonstrating, and giving a special course of lectures and practicals on vertebrate embryology to advanced students. But the teaching was effective: most of the students who took a zoology degree under Hill stopped on in the department, working on embryology, and I believe that more than half of them did so for so long a time that they ultimately took a D.Sc. Such a record is a remarkable testimony to Hill's powers as a teacher in rousing an enthusiasm for his subject in his able students. In this teaching Hill used lantern slides extensively and the collection now in the department gives evidence of the nature of the things taught, and the changes in interest there. Thus it is shown that Hill dealt with fossil vertebrates largely in his senior classes, gaining his knowledge—as a lantern slide of *Seymouria*, made before 1910, shows—from the original papers.

In 1913 Hill, with Professor Breslau and G. S. Sansom, made an expedition to Brazil, arriving there by a fortunate accident in October, which proved to be the breeding period of the marsupial *Didelphys*. They collected much useful material especially of other South American marsupials, later described.

At the end of the first world war Sir George Thane retired from the Chair of Anatomy in University College and was succeeded by Grafton Elliot Smith, a student of J. T. Wilson who had known Hill in Sydney in the 1890's. Elliot Smith recognized that the traditional anatomy of Medical Schools could be improved by taking over histology from the physiologists, and that—becoming 'human biology'—it could play an important part in the development of medical practice. Thus when the Rockefeller Foundation, developing its world wide schemes for the improvement of human health, sent representatives to London they consulted Elliot Smith, and shortly afterwards made a great grant to University College for the erection and endowment of a new department of anatomy. This department included a Chair of Embryology and Histology, to which J. P. Hill was appointed in 1921.

Much of his time during that year and the next was devoted to consideration of the departmental building then in progress, but in 1923 he moved into it, taking Mr Pittock with him.

Elliot Smith's idea in so appointing Hill was to free him from administration and much teaching so that his own research should go on the quicker; histology was to be dealt with by a 'subordinate'. But, very characteristically, Hill decided to do the work himself, giving a massive series of lectures and supervising the practical work, and especially the making of preparations used in it. These were extraordinarily good, of a different quality altogether from those then customarily used in 2nd M.B. classes.

But the new post made a gap of two years in Hill's list of publications, and the next work is Hill's first one on the embryology of Primates, its predecessor on *Tarsius* being a critical discussion of the literature. From this time on Hill was largely devoted to primate embryology, his Croonian lecture of 1932 being a treatment of the whole subject which reduced a vast literature to comprehensible order.

In 1930 T. T. Flynn, who had been a student of Hill's in Sydney, was appointed to the Chair of Zoology in Belfast, bringing with him vast collections of the eggs and embryos of monotremes and marsupials. These were added to those Hill possessed, and Flynn and Hill together began an extended investigation of early monotreme embryology to supplement 'Wilson and Hill', 1907.

Hill retired from his Chair at University College in 1938, but continued to work there as a 'Halley Stewart' Fellow for some time. The outbreak of war in 1939, followed by the evacuation of the College, raised the problem of housing Professor Hill's very large collection of slides and materials. The slides on which he was actually working were taken to Hill's residence at Finchley, the best of the slides and the spirit material were placed, along with valuable specimens from the British Museum of Natural History, in the cellars of the Rothschild Museum at Tring.

Thus Hill was enabled to continue his work, two vast papers by Flynn & Hill, published in 1939 and 1947, giving a most detailed account of the establishment of the primary germ layers in monotremes. After the war Hill received grants from the Wellcome Trustees, and continued to work in his own house and at the Department of Anatomy. This work continued almost uninterruptedly to the end of his life: he was busy at his microscope until the hour of his death, on 24 May 1954.

J. P. HILL'S SCIENTIFIC WORK

Hill's first six scientific papers, though accurate, deal with matters of no present interest, but the seventh, written in association with C. J. Martin, is the beginning of that investigation which occupied him for the rest of his life—for sixty years. It is a description of the structure of an embryo of *Platypus* (*Ornithorhynchus*) taken from an egg in the uterus nearly ready to be laid: the mother had been shot by one of the authors on 1 October 1894. The description is clear and precise and the figures admirable, and it was the first in the field, for the great collection made by Caldwell in 1884 had, and has,

yielded little beyond the celebrated cable 'Ornithorhynchus oviparous ovum meroblastic'.* The paper is full of new facts, and of novel interpretations, which time has justified.

The authors showed that the egg (with its shell) had greatly increased in volume since it was shed from the ovary. That the vesicle, of whose surface the embryo was part, completely filled the eggshell and was itself full of a nutrient solution, yolk, added to by the secretion of the uterine glands. That this rapid increase in volume by imbibition explains the delayed upgrowth of the embryo pressed by it against the shell, the albumen layer being of no thickness, and these qualities are therefore not found in the sauropsidan egg with its enormously greater yolk supply. They recognized that the blastoderm was mammalian in character with special resemblance to the marsupial vesicle.

Exactly one year later Hill read to the Linnean Society of New South Wales a paper describing the occurrence of an allantoic placenta in the marsupial *Perameles*, thus breaking down a universally recognized character of marsupials. Another characteristic of marsupials in general was the fact that only a single tooth in each jaw was changed. This fact produced an abundant literature, including many investigations into the dental lamina and the mode of formation of teeth in mammals in general. Much confusion resulted from this work, ending in the 'discovery' in mammals of a pre-milk and a post-permanent dentition which have no real existence.

With Wilson, Hill investigated this matter in *Perameles*, concluding that the marsupials had been derived from a stock with the normal milk and permanent dentitions of the Eutheria by the almost complete suppression of the milk series following on the adoption of the permanent attachment of the pouch young to the teat immediately after birth. Eleven years later Wilson and Hill found evidence of small transient teeth anterior to those discovered by Poulton in *Platypus*, which suggested that at some remote period the monotreme conformed to a normal mammalian pattern.

Next year came a full paper on the placentation of *Perameles*, followed by others dealing with the corresponding structure in other marsupials. Meanwhile Hill discovered that *Perameles* is born by rupturing the connective tissue between the median vagina and the perineum, the passage so formed healing up after each parturition. That this strange phenomenon was present in the marsupials of South America as well as those of Australia interested Hill for many years (from 1898-1926).

In a preliminary note in 1903 Hill, in association with J. T. Wilson, returned to monotreme embryology, and in 1907 published a great paper in which for the first time a connected account of the development up to and including the neurula stage was given. Unfortunately in this paper a 'yolk navel' was incorrectly taken to be a primitive knot, so that, despite the perfect accuracy of the individual description of stages, the general pattern

* The cable was sent by Caldwell in the field to the President of the Zoology Section of the British Association then meeting.

of development is confused. In 1915 Wilson & Hill corrected this error, and in 1939 and 1947 Hill, in association with T. T. Flynn, wrote a great monograph in which the establishment of the 'germ layers' is traced in the greatest possible detail.

In the 1907 paper Wilson & Hill re-examined the *Platypus* embryo described by Hill & Martin in 1894. They now produced a new interpretation of one obtrusive feature of that embryo. Here, on each side of the flat forebrain, there is an immense thick mass of cells lying between the ectoderm and a thin layer of scattered mesenchyme cells overlaying the endoderm. This mass was originally described as 'head plate' mesoderm, and carefully distinguished from the walls of the 'amnio-cardiale vesicles' and from the somitic mesoderm. It was recognized that it was homologous with a similar structure found by Selenka in the opossum. Now Wilson & Hill, examining an embryo somewhat younger, discovered that the mass was in continuity with the brain, and interpreted it as neural crest, the primordium of the gasserian ganglion, despite the fact that in the original later stage each head plate was of greater volume than the whole forebrain! Had they examined the next later developmental stage of a marsupial they could not have escaped the conclusion that the mass not only produced the ganglion but also gave rise to the whole mesoderm of the mandibular arch. That it was in fact neural crest mesoderm, now known to be of almost universal occurrence.

During the antipodeal winters of the years 1899-1901 and 1904 Hill bred *Dasyurus viverrinus* in captivity and thus made a great collection of eggs covering the period of early development. At that time the only publication dealing with that matter in marsupials was an old paper by Selenka (founded, in fact, entirely on abnormal materials of *Didelphys*), and a statement by Caldwell that the egg in the uterus was enclosed in a shell. In 1910 Hill gave a magnificent description of this material, beginning with the ovarian egg. During its passage down the Fallopian tube the fertilized ovum is surrounded by a layer of albumen, and this in turn by an egg shell, which are as Hill showed usual in marsupials and exactly comparable to those of *Platypus*, and imply their descent from a prototherian stock. Cleavage produces an upper ring of four cells which alone give rise to the embryo and the whole endoderm, and a lower ring which forms only the 'trophoectoderm' of which the remainder of the blastoderm is formed, the two regions meeting in a junction which remains recognizable for some time, until in fact the spread of the endoderm obscures it. Here the description ends, and in a lengthy but lucid discussion it is shown that, so far as our limited knowledge of early eutherian development permitted a judgment, the early marsupial process provides an admirable intermediate stage between a monotreme and that of a eutherian.

In two papers published in 1894 and 1897 W. Heape showed that primates had a definite oestral cycle in the behaviour of the female and in the reproductive apparatus. Not until 1903 did F. H. A. Marshall describe the oestrous cycle and the formation of the corpus luteum of the sheep, in a paper which first introduced the idea of hormonal control of this series of changes. It is



therefore remarkable that from 1899 to 1904 Hill should have kept such accurate records of the condition of his breeding *Dasyurus* from day to day (and preserved all the necessary parts of their genital apparatus) that it was possible to work out from them, in 1911-1913, the oestrous cycle and the changes in the corpus luteum and Graaffian follicle, and of the mammary glands, work done by Hill & O'Donoghue, and by the latter alone.

Much later, in 1926, with J. B. Gatenby, Hill described the growth and decline of the monotreme corpus luteum, showing that it contains two histologically distinct kinds of secretory cells, and that it is in full regression by the time the egg is laid, but conforms in general to the normal mammalian pattern.

In 1909, when he was writing up the *Dasyurus* development, Hill found that the then existing knowledge of the early stages of placentals did not reach the standards he desired. So he began to collect cat embryos. After examining several hundred females over a period of ten years Hill obtained a series which enabled him in collaboration with Dr Tribe to write what is in effect a cell lineage, which establishes the existence of a determinate cleavage back to the second or third cleavage generation. He had previously shown that this condition existed in *Dasyurus* from the fourth, and in *Didelphys* from the first cleavage.

In the cat, as in all Eutheria, one cell, passing early in development into the centre of a sphere of cells tightly packed round it, divided in such a way that its progeny remains in a tight packed mass attached to one pole of a sphaerical blastocyst undergoing enlargement by imbibing uterine secretion, exactly as do the corresponding stages of monotremes and marsupials, as Hill had earlier shown. In the cat, and in the members of the other two groups, two different types of cells arise in this cell mass, and by active migration the smaller of them spread outwards to line the blastoderm, and form the embryonic endoderm, whilst the remainder form the embryonic ectoderm, which in the cat, by disappearance of the thin layer of cells (Rauber's membrane) which overlies the embryo, comes to form part of the sphaerical outer surface of the blastocyst.

By this time Hill had effectively provided that knowledge of the early development of some member of each of the three classes of mammals necessary to answer the question which he posed to himself thirty years before. He had shown that not only the general plan, but many details of the early development of monotremes, marsupials and eutheria are only to be explained by an ultimate derivation of each of these stages from that before it. Thus he settled, I think for all time, a problem which arose when matters of phylogeny were first discussed, and which cannot be determined by consideration of adult morphology, nor from fossil materials.

But one possible uncertainty remained: the cat is not a very primitive eutherian, and its development, so comparable with that of a marsupial, may be a secondary condition, that found in 'rodents with inversion', and higher Primates, being antecedent to it in a phylogenetic sense. This latter

view was, in fact, the opinion predominantly held amongst embryologists in 1918. Thus it became necessary to investigate this problem, which was in essence the phylogeny of eutherian orders and some of their divisions. Hill had already begun to do so. In 1919 he discussed the literature about the development of *Tarsius*, and in 1922, with his old fellow student R. H. Burne, described the foetal membranes and placentation of a nearly full-term foetus of the lemur *Chiromys*. In 1928, with F. E. Ince and A. Subba Rau, he described the development of the foetal membranes of the Indian lemur *Loris*, showing that the animal resembled *Nycticebus*, earlier described by Hubrecht, but that the greatly extended knowledge acquired by the authors added the further evidence that, in general, the early development of *Loris* agreed with that which study of the cat had shown Hill to be the primitive eutherian pattern.

Meanwhile in 1915 Huber had described the early development of the white rat, and Hill's student, G. S. Sansom, in 1922 published an admirable detailed study of implantation in the vole *Microtus*. The story is added to by a joint description by Hill & Sansom of the implantation of the blastocyst of *Cavia*, the guinea-pig, not closely related to *Microtus* and *Rattus*. This paper, illustrated by superb micro-photographs made by Dr Sansom, was published in 1931. It showed that the three 'rodents with inversion' discussed in it exemplified three different ways in which the 'inversion' is brought about, and suggested that the condition, so far from being one primitive for mammals in general, was evidently a highly specialized device to secure an advantage to the rodents possessing it.

Thus, when in 1929 Hill delivered his Croonian lecture on 'The developmental history of the Primates' (published only on 19 May 1932), he was not troubled by the idea that the haemochorial type of placenta found in the higher monkeys was the original pattern, but could set out in order, and describe as consecutive 'evolutionary stages' of early development and placentation, the conditions found in lemurs, *Tarsius*, pithecoïds (platyrrhine and catarrhine monkeys) and anthropoids. These descriptions were all original, essentially every fact set out and discussed had been observed by the author himself in original materials. *Loris* was from materials collected by a friend of Dr Subba Rau, *Nycticebus* and *Tarsius* were based on Hubrecht's materials, *Hapale* was collected by Hill himself, other platyrrhines came from Selenka's collection, *Cebus* and *Chrysothrix* from Bluntschli's collection. Thus Hill gathered into his laboratory all the relevant materials known to exist in the world, and based his argument on direct personal knowledge, taking nothing for granted from the papers of any other worker. None the less he read all their papers with great conscientiousness and gave them full credit for all the facts they had recorded, differing from their interpretation apologetically when necessary. The whole work (133 quarto pages and 117 figures) is most impressive. It makes clear the nature of the problem, and then plunges directly into detail. Each stage of development of each animal is described and illustrated, earlier interpretations by other authors are dis-

passionately considered, then either accepted or refuted for reasons precisely stated. The whole is then summarized concisely in absolutely clear language, and finally definite conclusions based on the facts so set out are stated in the clearest possible way.

With this paper Hill in effect completed the investigation he had begun thirty-eight years before. He had shown, by a completely new line of evidence, that man shared with the anthropoid apes a unique, highly advanced type of early development and placentation, which could only have been derived from that found in catarrhine monkeys. That the catarrhine early development and placentation differs in small details from that of the platyrrhines in ways which render it unlikely that either of these groups could have been derived from the other. But both could have come from an animal unknown allied to the still living *Tarsius*. *Tarsius* in turn could only have come from the lemurs. The lemurid conditions are found as a pattern in many other primitive Eutheria, and are in effect foreshadowed by those in such a marsupial as *Dasyurus*. The lemur *Loris* even shows signs that its ancestors once possessed a yolk-laden egg. The marsupial egg has even more yolk, and in its passage down the oviduct becomes surrounded by an albumen layer and an egg shell, some kind of a placenta—even including an allantoic contribution—existing. ‘*Platypus*’ has a shell, ‘white’, and a large yolky egg, but the egg imbibes nutrient solution on its way down the genital tract, so that it is more advanced in a mammalian direction than that of any ‘sauropsidan’; but it is laid as an egg.

In University College Hill taught a number of men and women who proceeded to that Special Honours degree in zoology the course leading to which included (largely because he urged it on the ‘Board of Studies’ of the University) a written paper and practical examination in a ‘Special subject’ at a very high standard. In his hands this subject was ‘vertebrate embryology’, the field becoming more and more restricted, but the course requiring from the candidate a wide study of original papers and materials. Thus his students, on graduation, wished to settle for themselves questions which had come into their minds during his teaching, and Hill gave them materials on which they could work, usually on the development of some organ in which they were interested. Very commonly one or more of the resulting papers were published as joint work.

With Dr E. A. Fraser (1915) he wrote an elaborate account of the development of the thymus, epithelial bodies and thyroid in *Trichosurus*, to which Dr Fraser alone added an account of these structures in other marsupials. From the same materials she also showed that the marsupials alone amongst mammals possessed large ‘anterior head cavities’ from whose walls the eye muscles innervated by the oculomotor nerve arise. In this they agreed with lower vertebrates from elasmobranchs onward.

Somewhat later (1917) Dr K. M. Parker, working on the development of

the marsupial pituitary body, showed that these head cavities arose as outgrowths of the extreme anterior end of the fore gut, from the prochordal plate. Much earlier it had been recognized that the anterior head cavities were connected with one another by a canal in many lower vertebrates.

In 1924 Hill, with Dr Tribe, emphatically states his belief that the prochordal plate is a surviving relic of a premetameric stage of vertebrate development, and presumably ancestry. It was thus a structure of very great interest and Hill returned to its consideration in human embryos, with Dr Florian, in a paper published in 1931.

For some years, as student and colleague, Hill had the pleasure of a scientific association with his daughter, C. J. Hill, who in 1933 published an account of the histology of the monotreme oviduct during gestation, to which he added an account of the development of the egg shell.

With G. R. de Beer, who succeeded him in the Department of Anatomy, Hill described the vestiges of the egg tooth and analogous structures found in marsupials.

Thus, taken as a whole, the work of J. P. Hill and his school has made a unique contribution to descriptive embryology. To it we owe what is essentially all our knowledge of the early development of the monotreme and marsupial mammals, and much about their organogeny which cannot be found elsewhere. It is Hill's work that has established beyond dispute that Monotreme, Marsupial and Eutherian represent essentially definite evolutionary stages through which the higher mammals have all passed, and to him we owe the best evidence which exists to show that we ourselves, man, have been derived from ancestors which, traced backward, are anthropoid, pithecoïd, tarsioid and lemurine in the plan and details of their early development. Though this work is descriptive the discussion of detail is full of suggestions as to the functional significance of unusual conditions, of the retention, for example, of a flat brain plate in marsupial embryos to a stage of morphological and histological development found in sauropsids and eutherian mammals long after closure of the brain vesicles.

Hill was a teacher who took his work very seriously. When I succeeded him at University College he lent me his notes for the Intermediate course in zoology. These showed that in his first session, 1906-7, he had written out not notes but a full hour's lecture for each meeting of the class. He revised these every year, correcting anything which he felt to be misleading, or out of date. But very often the effect was to introduce more matter into each lecture. In fifteen years they were in effect an unusually detailed text-book, including a mass of morphological facts impossible of immediate comprehension. The students' only hope was to get it all down verbatim, and then study it at leisure. But to the morphology some physiology came to be added. His senior lectures on both vertebrates and invertebrates were similar, full of detail and kept entirely up to date. They were illustrated by lantern slides and by splendid blackboard drawings, made in advance and sometimes labelled 'keep', so that they went on from one

meeting to the next. When he came to teach histology to medical students he followed the same plan.

Hill was thus a very busy man, but he worked long hours. He was in the department from 9 a.m. to 6 p.m. every day except Saturday, when he left at midday. And he spent the whole of this long day at work, for he usually lunched on sandwiches eaten in his laboratory, and seldom visited the 'Professors' Smoke Room'. At any time when you went to see him you expected to find him sitting at a bench with a microscope, sometimes with a binocular embedding an embryo and determining the proper plane of section, usually with a set of slides hunting some fact. He often took slides home, where he kept a microscope in his study, and there he read the literature, regularly working steadily up to midnight. But he was a keen golfer, playing at most weekends and during the summer vacation, when he took a six weeks' holiday with his family, sometimes at Elie in Fifeshire, near his old home, or latterly more frequently at Seaford in Sussex. His golf appears to have been good, and he believed in its therapeutic effect.

Nearly all his published papers were written in collaboration, at first with a senior man, especially J. T. Wilson; after he came to London he was the senior author. This collaboration was genuine, both authors studied the materials as intensively as if each were writing independently, and they discussed the work in very great detail, down to the point of whether an individual cell should be included in the neural plate of a flat embryo being graphically reconstructed, or not. Thus Hill secured a very high accuracy of statement of fact. Then arose discussion of the significance of the facts so discovered. Here also both authors contributed; in the early days, as Hill himself records, it was reputed that the discussions between Wilson and Hill could be heard a quarter of a mile away, and as Elliot Smith said to me, these two 'wrangled like a pair of Scots'. But such discussion of particular problems, known to both authors not only from a study of sections, but of the whole literature, seems to have been useful to 'J.P.' (as he was always known at University College) in enabling him to make up his mind.

Then came the drafting of the text, written and corrected until it had become lucid and precise. There is a story that when E. S. Goodrich was applying for appointment as professor in Oxford, Hill and F. J. Cole drew up a document which was widely signed and sent to the Electors. Cole drafted it with immense care, and stopped with Hill to discuss its wording. After dinner Cole gave his draft to Hill, who read it, said nothing, but some time later suddenly remarked—'damn it, man, at least let's have that comma out.' Determination of the text of a joint paper was a similar process.

In contrast to this pernickety attitude toward the written word he was apt at times to lapse into complete informality in speech. On one occasion he put his head round the door of the senior laboratory and asked a class within, 'have ye finished the bleeding things with legs?'—thus referring to the Arthropoda, a phylum for which he had a distaste.

Hill possessed more knowledge of embryology, and probably of zoology in

general, than any of his contemporaries in England, but his contributions to public discussion were always marked by extreme diffidence. In work with research students he was wont to express a view with great finality, but was always ready to alter it on satisfactory evidence being shown.

Hill had a very great personal charm of a kind which is difficult to make evident to anyone who did not know him. I do not think he knew that he possessed it: certainly he never traded on it, and some people did not recognize its existence. It depended in part on that complete sincerity which informed all his scientific work, accompanied by a lack of any sense of urgency: that a thing should be done as well as possible far outweighed any sense that, if its completion should be deferred from year to year it might (even if ultimately achieved) appear after general interest had passed far beyond the matter involved. His relation to his research students and associates was close, and, after a first period, a relation of equals, with whom everything must be discussed in the greatest detail. Everything in which they were concerned was present in Hill's mind, and led him to make interjections deprived of their context. 'I see you've got your head back and I thought it would take two men and a boy,'—spoken from a slightly opened door to a research student who, rearranging the departmental museum, had had difficulty in reinstating a hippopotamus skull on an upper shelf.

Such an account of Hill's life may give an impression of a somewhat inhuman character, but any such belief is entirely mistaken. Hill thoroughly enjoyed the expeditions in Australia, and later in Brazil, when he collected materials and lived in camp with friends. Writing of those days in New South Wales Hill says 'those were years of intensive activity, with the days often prolonged far into the night and relieved by 'working holidays' in the Blue Mountains during the long summer vacations and by camping expeditions into the bush in search of monotreme and marsupial material during the short winter breaks. These camping trips were memorable and joyous occasions when we reverted to the primitive life—shot and cooked our own food, hunted by day and yarned over the camp fire by night.' I remember walking through the departmental museum with him when he suddenly pointed to a mounted skeleton of an agouti and without any preface remarked, 'that provided the second best soup I ever made.'

Hill's tenure of the Jodrell Chair ended very soon after the first world war and covered a time when the College funds were very small, when an additional binocular microscope was a very serious expenditure, and when there were (except for the small Derby Scholarship) no funds available for the support of research students. Only late in J.P.'s time did the parents of a research student who died young, Percy MacGregor, give to Hill a small scholarship to support a research student. He was, perhaps, a little unworldly in his apparent belief that all new graduates could be expected to continue to work with him without any grant. And with his women students not every post was suitable. But he did his best to make financial

provision for those whose work was good. After the war the D.S.I.R. came to the rescue.

Thus, that so many of those who took their degree under Hill stopped on in the department and worked with him on his material is a testimony of the affection which he inspired in them. They were worked hard, they had to be prepared to discuss in detail the evidence for every point in a graphical reconstruction. They were expected to have read all the literature, and to make use of it. And when the work was done and written up the hope that further material would come from some unknown source 'further to paint the lily' might defer publication from one year to another, so that, for instance, what was really conclusive evidence of the neural crest origin of the marsupial mandibular arch, recognized in 1918, still lacks publication.

Hill's judgment of men was almost always kindly: he recognized before others the uncanny genius of Robert Broom, who of all men was the most unlike himself in scientific method. He had an admiration of the work of Spemann and was delighted to recognize in him a man who had himself cut every section he had used in his work on the embryology of newts. But although he was peculiarly well fitted to do such experimental work he never did so. He respected and admired Hubrecht, although he steadily corrected his conclusions until nothing was left, and his relationship to J. T. Wilson, shown so well in his obituary notice of his old friend, was delightful.

One of Hill's former colleagues most justly sets out traits which won their affectionate regards for the man himself. His courtesy and unfailing readiness to give time to a junior colleague who stood in need of the illumination of J.P.'s keen mind. That he could meet on their own ground and in a foursome beat his students in a staff-student golf match. That in his latter years he refreshed his spirit by listening to Beethoven late at night at the end of his day's work. And the pleasure of visiting him and his family in a house significantly called 'Kanimbla', the Australian name of a beautiful former camping ground in New South Wales which means 'Valley of the Kangaroos'.

These things add to our understanding of a man whose life was devoted to a single problem, pursued over a period of very nearly sixty years.

Hill was elected to the Royal Society in 1913, served on the Council 1925-27, and was Darwin Medallist in 1940. He was D.Sc.(Hon.) of the Universities of Dublin and Belfast, and Honorary Member of many foreign societies, including the National Academy of Sciences, U.S.A. (1940), Hollandsche Maatschappij der Wetenschappen, Haarlem (1925), the Société Royale de Bohême, the Société Portugaise des Sciences Naturelles (1921), and the Royal Society of New South Wales (1929) and the Linnean Society of New South Wales (1923). He was also a Founder Member of the International Institute of Embryology. He was awarded the Mueller Medal of the Australian Association for the Advancement of Science in 1906, and the Linnean Gold Medal in 1930.

This biographical sketch of J. P. Hill's life and work is founded on knowledge gained from a forty-four year friendship. But it has been controlled,

added to and greatly improved by the advice of many of his students and associates.

To Dr Catherine J. Hill (Mrs Kirkham Jones), Dr Elizabeth Fraser, Dr Margaret Tribe and Dr Katherine M. Parker (Mrs Watson) I owe some family history, many reminiscences and a check of my memory of J.P.'s activities. Professor F. J. Cole, who alone remembers the days of Professor Howes's department at South Kensington, gave me some knowledge of that stage in Hill's career. Professor P. D. F. Murray gave me some records of Sydney. I am indebted to Drs Abercrombie and Newth who read my account of Hill's scientific work.

The photograph illustrating this notice was taken by F. J. Pittock in 1935.

D. M. S. WATSON

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T. Woodruff

FREDERIC WOOD JONES

1879-1954

FREDERIC WOOD JONES was born in London on 23 January 1879, the son of an architect of Welsh descent. As a boy he was a keen entomologist and showed an unusual interest in all branches of natural history. These pursuits engaged much of his attention during his school days and, with a number of interruptions due to ill-health, no doubt accounted for the fact that his school career was quite undistinguished, and that he also found some difficulty in passing the examination for the London University Matriculation. He finally matriculated in 1897 and entered the London Hospital as a medical student. It was here that he first showed evidence of his intellectual ability, for he won a succession of scholarships and prizes in anatomy, physiology and clinical medicine. It was at the London Hospital, also, that Wood Jones came under the influence of Chalmers Mitchell and Arthur Keith, to both of whom he later recorded his debt of gratitude for their stimulating influence. He qualified with the degrees of M.B., B.S. in 1904, and even while a student he had already contributed to the *Proceedings of the Journal of Anatomy & Physiology* (as it was then called) short notes on the musculature of the bladder and urethra, the development of the fundus of the stomach, and a method of reconstruction of a human embryo by photographic reproductions of serial sections. These early publications showed a certain originality of approach which was later to become a very characteristic quality of Wood Jones's work. It is not surprising, therefore, that his teachers and friends hoped that, on qualification, he would settle down to an academic career in London. But, against all their advice, he left England in January 1905 to take up an appointment as Medical Officer to the Eastern Extension Telegraph Company in the Far East, and, after a short stay in Singapore, sailed for the Cocos-Keeling Islands. We do not know what decided him on this course, but it clearly fits into the pattern of the whole of his subsequent career, for he was essentially a wanderer and a seeker after adventure; indeed, he never stayed in any of the numerous positions which he held for more than a few years.

In the Cocos-Keeling Atoll (which, incidentally, in 1836 was the first atoll to be visited by Darwin during the voyage of the *Beagle*), Wood Jones stayed for fifteen months on the little island of Pulau Tikus, a strip of coral land less than a mile long which he shared with about a dozen Europeans, a few Chinese servants and three Malay boatmen. As he said in one of his books, the outlets for medical enthusiasm here were strictly limited and he had

abundant time to study the reef-building corals and to make collections of the local flora and fauna. He also took part in a number of cable expeditions among the islands of Malaysia, including short visits to Australia and New Zealand and a double journey of the whole extent of the Great Barrier Reef, and finally returned to England in 1907. No sooner had he done so than he was invited by Elliot Smith to assist in field anthropological studies on behalf of the Egyptian Government Archaeological Survey of Nubia. Here he remained until the summer of 1908, and the results of his studies are recorded in two of the bulletins published by the Survey. These studies, which were carried out with systematic thoroughness, dealt with a variety of subjects such as the mode of burial and treatment of the body from the Predynastic Period of Egypt to the Ptolemaic Period, the anatomical and sexual variations in the skeletal material examined, and the pathological conditions which were encountered in the latter. It is clear that the collection of all this material and the completion of its study in less than a year must have involved a great deal of concentrated work. The publications also demonstrate the width of Wood Jones's knowledge even in these early years.

On his return to England, Wood Jones turned his attention to an analysis of the data which he had accumulated during this stay in the Cocos-Keeling Islands, and published the results of his investigations in a remarkable book *Coral and atolls* which appeared in 1910. It was remarkable because it embodied an illuminating study of coral growth and atoll formation by a man who had had no specialized training in zoology and who had actually completed all the field studies on which his conclusions were based in little more than a year and while still in his twenties. As a matter of fact, the book received a somewhat unfavourable reception from certain quarters, mainly because of his interpretations of atoll and reef formation. But work on the problems of coral reefs had for many years been riddled and confused with controversy and contradictory opinions, and it is perhaps not surprising, therefore, that Wood Jones's interpretations came in for their own share of criticism from over-zealous protagonists of different views. For a present-day and authoritative assessment of *Coral and atolls*, I am indebted to Professor C. M. Yonge, F.R.S. (Leader of the Great Barrier Reef Expedition, 1928-29), who has kindly permitted me to reproduce his commentary in the immediately following paragraph.

'Wood Jones's great contribution to the study of coral reefs lies in his consideration of corals as living things interacting with the environment. At the time when he published *Coral and atolls* much work had been done on the systematics of corals, but this was almost entirely carried out in museums on dried skeletons and very little had been done on corals as animals. There was also the still debateable question as to the manner in which barrier reefs and atolls had been formed. During the period he spent on Cocos-Keeling Atoll Wood Jones had the opportunity of day-to-day observation which had not been possible for most of his predecessors, and when the members of the Great Barrier Reef Expedition began work in the summer of 1928 it was to

his book that they turned for the best account then available of a living coral reef. What particularly impressed Wood Jones was the extent to which the form of a coral colony is moulded by environmental influences. Indeed, he believed that many of the numerous species created by the systematists were in reality growth forms of a single species. Thus, he went so far as to suppose that all forms of *Pocillopora* found on the atoll were one and the same species. This matter was investigated in great detail by Professor T. A. Stephenson, F.R.S., when in charge of the reef party of the Great Barrier Reef Expedition, and, while acknowledging the very great stimulus he received from Wood Jones's work, he showed that the latter actually went too far in this direction; for, although it is quite true that many growth forms of individual species do exist, there are also many true species that can be recognized as such in the field. Wood Jones also drew attention to the importance of the factor of sedimentation, which he considered could alter the appearance of coral to a very considerable extent. Here again his work stimulated later investigations made by S. M. Marshall and A. P. Orr during the course of the Great Barrier Reef Expedition. There is now no doubt that corals can rid themselves of sediment to a greater extent than Wood Jones realized, while at least some of the effects which he attributed to sediment are probably due to exposure at low tide. Finally, on the broad question of the form of atolls Wood Jones considered that, at any rate in the case of Cocos-Keeling, this was due entirely to the action of currents, and he illustrated his views with a series of explanatory diagrams. In this he followed Semper and these views undoubtedly contain some measure of the truth, but here he would seem to have failed to appreciate the positive reaction of the coral colonies growing against the action of wind and weather. However, it is only fair to say that finality has by no means been reached on this matter. It may be said in conclusion that Wood Jones certainly produced probably the most stimulating book on corals and on coral reefs that has ever been published. By describing the growth, repair and death of coral colonies he focused attention on the fact that they are animals, and, although he did somewhat over-emphasize the importance of the environment on form, he went no further in that direction than the systematists had done in the other. The truth lies well between the two extremes. He is certainly assured of an honoured place in the history of the investigations of corals and coral reefs.'

Wood Jones's book on coral and atolls also contains useful chapters on the history of the Cocos-Keeling Islands and on the littoral and marine life of the Cocos-Keeling Atoll, as well as appendices which include a complete list of the local fauna and flora. Some of this information was further elaborated in publications in the *Proceedings of the Zoological Society* and elsewhere. The manuscript of the book was submitted as a thesis for the D.Sc. degree of London University, and this degree was conferred in 1910. One very important result of Wood Jones's visit to the Cocos-Keeling Islands must be mentioned here; in 1910 he married Gertrude Clunies-Ross, daughter of George Clunies-Ross who was Governor of the Islands. There followed

a life-long partnership, a partnership to which he continually paid tribute by dedicating to his wife almost every one of his books published thereafter.

In 1909 Wood Jones was invited to Manchester University to be Lecturer in Anatomy under Elliot Smith, and in 1910 he was appointed Senior Demonstrator at St Thomas's Hospital Medical School. In 1912 he transferred to the London School of Medicine for Women where, in 1914, his position was raised to the status of a professorship. During these few years he published interesting articles on the lesions caused by judicial hanging (with recommendations for improvements of technique in order to ensure instantaneous death), a variety of short anatomical papers on such subjects as the relation of the limb plexuses to the ribs and vertebral column, the surgical anatomy of the rectum and of the meningeal vessels, the embryological basis of extroversion of the bladder, the functional history of the coelom and diaphragm, and an illuminating account of the phases in the reproductive cycle of the female mole. This last paper initiated a series of studies on the comparative anatomy of the genitalia of mammals and reptiles, which were summarized in an Arris and Gale Lecture at the Royal College of Surgeons in 1915 on 'The influence of the arboreal habit in the evolution of the reproductive system'. It was on this occasion that Wood Jones first came to public notice as a lecturer of unusual ability, and in his subsequent career he acquired a very considerable reputation for the ease and fluency of his delivery. Curiously enough, although he was undoubtedly pre-eminent as a lecturer, he was never outstanding in spontaneous public discussions. The reason for this contrast is that he was equipped with unusual powers of memorization, and the present writer has been told by a member of one of his audiences, who on that occasion had the prepared script of the lecture before him, that the latter was delivered by Wood Jones from memory with an absolutely word-by-word accuracy.

Wood Jones's studies of the comparative anatomy of the Primates convinced him of the fundamental importance of an arboreal habitat in the determination of their major morphological characteristics, and he expounded this general thesis in a most stimulating book *Arboreal man* which appeared in 1916. This book is in no way a systematic or intensive work—it is rather a series of well-written and attractive essays based on a number of lectures. But it may justly be regarded as a little 'classic' in the study of the comparative anatomy of man and the other Primates, which even to-day still provides an excellent introduction to the subject. Wood Jones stressed and elaborated the important point (which was, in fact, not entirely new) that the requirements of an arboreal habitat play a dominant role in preventing structural specializations and that the retention of so many primitive mammalian characters in the Primates is explicable on this basis. He later extended these ideas to a more direct consideration of man's place among the Primates, and in *The problem of man's ancestry* (1918) we see the first expression of a trend of thought which became increasingly manifest as the years went

by. Briefly, the gist of his thesis was that in certain respects man retains anatomical characters which are more primitive than the equivalent characters in most other Primates, and therefore (he concluded) the phylogenetic sequence which ultimately led to man must have had a very long period of evolutionary independence. His lines of argument appeared to imply three major assumptions (though he never explicitly stated these)—that individual characters are not susceptible to evolutionary reversal, that the early appearance of a character in embryonic development indicates a correspondingly early development in phylogenetic development, and that the phylogenetically earlier representatives of the major subdivisions of the Primates showed specializations similar to their modern representatives. Although none of these assumptions can be accepted as entirely valid, this is not to say that Wood Jones's arguments were in any sense sterile; on the contrary, he brought up many new points and novel interpretations (particularly in the relation of structure to function) which were very fertile in the discussions which they provoked among his contemporaries. His arguments finally led him on to suppose that man has a closer relationship to the aberrant Primate *Tarsius* than to any of the other groups of Primates, and by some this conception was given an importance which it perhaps did not altogether deserve by being called the 'Tarsian (or Tarsioid) hypothesis'. For the fact is that Wood Jones never developed and elaborated this thesis in sufficient detail to establish it as a real alternative to other theories of man's origin; nor did he attempt by going into particulars to specify the characters of the supposed ancestral 'Tarsioid' from which he believed the human family to have been derived. The subject led to a somewhat lively symposium at a meeting of the Zoological Society in 1920, when the hypothesis met with rather severe criticism.

During the First World War, Wood Jones took a commission as Captain in the Royal Army Medical Corps and was stationed at the Military Orthopaedic Hospital, Shepherd's Bush, and at the end of the war he was appointed a Registrar of Neurology under the Pensions Board. Two publications resulted from his war work—a paper on voluntary muscular movements in cases of nerve lesions in which he demonstrated the great variety of 'trick movements' whereby patients with partial motor paralysis are able to effect movements normally determined by the paralyzed muscles, and a book (published in 1920) on *The principles of anatomy as seen in the hand*. The latter was based on a series of lectures given as a part of the course of instruction to officers of the R.A.M.C. at the Special Military Surgical Hospital, Shepherd's Bush, and provided an excellent introduction to the study of the whole subject of human anatomy. It was widely used by students of anatomy, and many (including the present writer) derived an inspiration from it which was of great service to them in their later studies. A second edition of the book appeared many years later, in 1942, but it was perhaps less satisfying since it did not incorporate some of the important advances which had been made as the result of numerous researches in the intervening period.

At the end of 1919, Wood Jones went to Australia to take the Chair of Anatomy at Adelaide University, where he remained for eight years. Here he found abundant opportunities for expeditions and field studies, activities which always attracted him far more than laboratory research. It is perhaps well to emphasize here that he was not really the laboratory type of worker. It is true that in his earliest papers he used the microscope for some of his embryological studies, but thereafter, except for occasional dissections in his studies of comparative anatomy (and even these were never pursued very far or very systematically), almost all his original work was the result of field observations or the macroscopic examination of zoological specimens. As he himself recorded, the type of research work which he undertook was largely determined by the circumstances of his wanderings, and this certainly accounts for its unusual variety. He took part in several expeditions in South Australia between 1920 and 1927; for example he crossed Cooper Creek in 1920 and again in 1925, and he reached Lake Eyre by journeys from the west in 1921 and 1922. In 1923 he visited Lake Phillipson, and in the same year he made a further journey to the Stuart Ranges. On these expeditions extensive zoological, botanical and anthropological collections were made (some of which led to the discovery of several new marsupial species as well as many new species of invertebrates) and the results were published in a series of papers which appeared in the *Records of the South Australia Museum* and in the *Transactions of the Royal Society of South Australia*. At this time also, he published a revision of the South Australian jerboa mice and a series of short papers on the external characters of pouch embryos of marsupials, as well as a number of observations on the habits and distribution of the local fauna. Apart from his main expeditions, Wood Jones organized and conducted small working parties of students and others, camping on a number of the offshore islands in order to study ecological problems; these short excursions, as will be readily understood, were exceedingly popular and provided excellent opportunities for training students in field work. Between 1923 and 1925 a systematic catalogue of the mammals of South Australia was published in a series of handbooks of the fauna of South Australia issued by the South Australian branch of the British Science Guild. This is perhaps one of Wood Jones's best works on comparative anatomy—it was certainly his most complete and systematic study. The books contain a wealth of information presented in a most attractive style, and are of particular value in relation to the fact that some of the species are not only rare, but are approaching extinction. An outstanding feature of this work is the series of 311 illustrations which were all drawn by the author. Wood Jones was remarkably proficient as an anatomical artist and, in fact, all his books and scientific papers were almost entirely illustrated by himself. The style of his line drawings was not only aesthetically pleasing—they were also notable for their accuracy and clarity. It is evident that this systematic survey of the South Australian mammals was based on extensive studies of the relevant literature as well as the result of much detailed personal observation, and it would be difficult to find a

similar type of handbook which combines so much systematic information with so many original contributions.

In 1927 Wood Jones accepted an invitation to fill the Rockefeller Chair of Anthropology in the University of Hawaii, where he remained for two years. It was here that he began a study of the cranial characters of the local varieties of the domestic dog found in different islands of the Pacific. Remains of these animals are commonly found interred with ancient burials of local chiefs, and Wood Jones conceived the idea that, since they were uncontaminated by interbreeding with the European varieties which were later introduced into the various areas, they would provide excellent material for the differential analysis of cranial characters which might eventually supply sufficient data for determining their geographical origin (and thus the geographical origin of the early colonists of the islands who brought their domestic dogs with them). Unfortunately he did not complete this systematic study, so that the results were inconclusive. The same applies to his series of papers on the 'non-metrical' morphological characters of the human skull in different races (e.g. the Hawaiians, the prehistoric inhabitants of Guam, the Tasmanians and the Chinese). The idea behind this survey was attractive, but the results were never finally collated to permit any definite conclusions regarding racial affinities. It was at this time, also, that Wood Jones published two books which gained considerable popularity. One, *The matrix of the mind* (written in collaboration with S. D. Porteus) was a general account of the comparative and functional anatomy of the brain. There was nothing particularly new in this book (and it was not based on original observations), but it presented in an unusually attractive and stimulating style an elementary account of what for the student is certainly the most difficult system of the body to understand. The other book was entitled *Man's place among the mammals*. This work comprises a general systematic and well-illustrated account of the comparative anatomy of the Primates which might well have become a standard text-book on the subject for students of anthropology. Unfortunately, however, its value from this point of view was rather seriously affected by the introduction of polemical forms of argument and of somewhat personal animadversions on opinions expressed by fellow anatomists. No doubt some of these opinions were deserving of criticism, but the author tended to combine criticism with censure in a manner not entirely suitable for a work of this kind.

In January 1930, Wood Jones returned once more to Australia, this time to occupy the Chair of Anatomy at the University of Melbourne. During the next few years he made several visits to the semi-arid 'mallee country' in the northern part of the state of Victoria and helped to found the McCoy Society for Field Investigation and Research. Many of the islands of the Bass Strait (and also islands as far to the west as Spencer Gulf) were also visited and extensive collections made, and the results of these expeditions were published in *Reports of the McCoy Society* and elsewhere. In spite of all this field work, Wood Jones still found time for a number of papers on strictly ana-

tomical subjects. One of the most interesting of these was based on his re-examination of the skeletal characters of the fossil marsupial which had been described thirty years previously by Baldwin Spencer and named by him *Wynyardia*. Wood Jones demonstrated by careful comparisons that this extinct genus is closely allied to *Trichosurus* and is in no sense the annectant form between the Polyprotodontia and Diprotodontia as some had supposed.

Apart from his anatomical activities, Wood Jones not infrequently made excursions into other fields of study, from which he usually brought back novel ideas for consideration. Thus, he made a study (with Dr T. D. Campbell) of supposed coliths from the Central Australian tablelands, and demonstrated that the apparently purposeful chipping could be explained by the impact of wind-driven sand, grit and pebbles. He also contributed an interesting note on the ordered arrangements of stones in some of the 'clay-pans' of Central Australia, concluding that they marked the position of old corroboree grounds for the subincision rites of the aborigines that once lived there. Some of his most interesting and productive side-lines of study were ornithological, but it is not easy to summarize all the results of his observations in this field for they were diverse and appeared in scattered publications. For example, he published in the *Transactions of the Royal Society of South Australia* (1926) an account of his observations during sea voyages of the flight of sea-birds, noting that in passing from the Equator towards either Pole a very similar series of avian types is encountered as successive zones of temperature are traversed. Thus, towards the Poles, as the air becomes colder and denser, the wing plane of the different types relatively to the body weight diminishes, the birds being efficient gliders in their standard atmospheres with considerably reduced planes. He sought to explain this on aerodynamic principles, and remarked the curious fact that the tendency for wing area to decrease relatively to body weight culminates at both Poles in the production of flightless birds—the southern penguins and the northern auks. He also published a number of short notes on the breeding of sea birds on the islands off the coast of Victoria, and a brief account of the olfactory organs of the Tubinares. The unusual size of the olfactory chambers in these birds had been noted several years previously by W. P. Pycraft, but Wood Jones gave further details of their anatomy, describing the elaborate formation of three scroll-like turbinals and the large olfactory bulbs. He associated this exaggerated development of the olfactory apparatus with the fact that the Tubinares are characterized by a very distinct odour derived from cutaneous scent glands.

At the end of 1937 Wood Jones left Australia for England where he had accepted an offer to occupy the Chair of Anatomy at Manchester. During the next few years he continued to write a number of descriptive papers dealing with comparative anatomy (for example, he studied the curious modifications of the 'thumb' of the giant panda and reviewed the comparative anatomy of the soft palate) and he also undertook the arduous task of editing the seventh edition of Buchanan's *Manual of anatomy*. In 1944 he

published a stimulating book on *Structure and function as seen in the foot*. This book, which is of considerable importance to orthopaedic surgeons, is enlivened by many references to the writings of old anatomists. It may be mentioned here that Wood Jones had a particular interest in the history of anatomical discovery, and frequently took the opportunity to point out that many of the acute observations of the early anatomists had evidently been forgotten since they were so frequently overlooked in modern treatises. He had a magnificent library of old anatomical works, including some first editions which were of great value. In 1945, Wood Jones assumed his last academic office, that of the Sir William Collins Professor of Human and Comparative Anatomy at the Royal College of Surgeons of England and Conservator of the Museum. He had been elected to the Fellowship of the College in 1930.

Towards the latter part of his career, Wood Jones developed in much more detail his personal convictions (which had become increasingly evident in his previous writings) regarding some of the general problems of evolution. For example in a series of books, *Design and purpose* (1942), *Habit and heritage* (1943), *Hallmarks of humanity* (1948) and *Trends of life* (1953), he affirmed his adherence to a somewhat modified Lamarckian interpretation of evolution and at the same time expressed himself as an uncompromising 'anti-Darwinian'. He also argued vigorously against any close phylogenetic relationship between man and other Primates (with the solitary exception of *Tarsius* which he still held to be the only surviving representative of the ancestral stock from which man was originally derived as far back as Eocene times). It will be generally agreed that here he carried his arguments too far and sometimes rather seriously overstated the evidence on which he relied; it also seems from his phraseology and his needlessly caustic disparagement of some of the great biologists of past days that he had developed a strongly emotional attitude towards the whole conception of evolution, particularly in reference to the natural selection of heritable variations. For example, he refers to 'the stupid term "the survival of the fittest"' and to 'the modern fashion of genetics' (this last in a book published in 1953), and he gave the impression of seeking to discredit the names of Darwin and T. H. Huxley by unduly harping on certain of their anatomical statements which, in the light of present-day knowledge, no doubt do require some revision. But, in spite of this biased approach, his controversial books and essays were always well worth reading, for they contained a great deal of new or little-known information, and it was impossible not to admire the dialectical skill with which he marshalled his arguments—even if it appeared that his evidence was sometimes presented with an almost deceptive ingenuity. However, the fact is that Wood Jones sometimes gave the impression from his style of writing that he regarded the whole problem of evolution and its mechanism almost as an article of faith rather than as a thesis which, although already supported by a considerable mass of scientific evidence, is, of course, susceptible to further enquiry by the rigid methods of scientific investigation. He was obviously impressed with the

precision and detail of functional adaptations in the animal world, as presumably all zoologists are. But, unlike zoologists in general, he allowed himself to be so overwhelmed with this outstanding characteristic of living organisms that he was content to ascribe all adaptations to an obscure and undefined kind of directive force, and simply to leave it at that. There was, indeed, a distinct element of mysticism in his attitude to some of the fundamental problems of organic life and, as a result, his more popular writings aroused considerable response from the class of general reader to whom the methods and philosophy of science make little appeal.

There is no doubt that Wood Jones's wide reputation as an anatomist gained much from his skill as a writer, as also from his astuteness in controversy. Indeed, there can have been few anatomists whose writings were more widely read. He wrote with great ease and fluency, he had a most attractive literary style, and he was remarkably persuasive in the formulation of his arguments. Many of his lectures and orations were collected in book form, and he also wrote delightful essays on general subjects which he published in separate articles and in two books *Unscientific essays* (1924) and *Unscientific excursions* (1934). These two volumes of essays are mostly based on apparent trivialities culled from the many observations and experiences of his extensive travels. But each one of them contains some original point of view or some novel idea which intrigues the reader and stimulates a train of thought. They also bear witness to the author's wide range of interests, and to his capacity for seeing so many apparently ordinary things from a slightly unusual angle and thereby rendering them to that extent extraordinary.

It is a rather remarkable fact that, although Wood Jones never occupied a position in any University for more than a few years, he always seems to have left behind him an abiding tradition (almost, one might say, a 'legendary' tradition) of his outstanding and vigorous personality. In Australia he was the recipient of two honorary degrees and several medals, and because of his reputation as a lecturer he was called on to deliver many memorial lectures and orations. As further evidence of the esteem in which he was held, a full-length oil painting of him now hangs in the entrance hall of the Australasian College of Surgeons in Melbourne. And, although he only occupied the Chair of Anatomy at Manchester University for seven years, he left such a strong impression on the student body that they subscribed to institute an annual 'Wood Jones Lecture'—surely a most unusual tribute after so short a tenure of office. Incidentally, Wood Jones himself gave the first of these lectures in 1946.

That Wood Jones during his academic career was able to complete such an extensive list of publications (he was the author of fifteen books as well as of very numerous scientific papers, lectures and articles) seems surprising in view of his many activities in the field and his academic responsibilities. He was indeed a man of great mental energy, and (as already noted) he had a very facile pen. But he was also by nature what is sometimes called an 'individualist', and he focused his attention almost entirely on the sort of

work in which he himself had a special interest; he was not concerned to develop or re-organize any of the various departments of anatomy of which he was the director, he did not take a very active part in the usual routine of University administration, he founded no 'school' of young anatomists, the supervision and direction of which would have distracted him from his own researches, and he seems to have limited his social activities to a bare minimum. A man of restless curiosity and of strong opinions, he sometimes gave an appearance of intolerance towards those who disagreed with him. He was also very critical of the development of new technical methods in anatomical research, even going so far as to imply that any method but that of scalpel and forceps (and perhaps the low power of the microscope) was beyond the proper domain of the science of anatomy. It is little wonder, therefore, that he was out of sympathy with the younger generation of anatomists; so much so, indeed, that he occasionally allowed himself to be almost too brusque in his attitude towards them and their work. But although he was possessed of a certain natural acerbity of temperament which impelled him to express his views in somewhat unaccommodating terms, Wood Jones certainly contributed his full share (and more) to the store of comparative anatomical knowledge, and the stimulus which he gave to his students and contemporaries by his lectures and writings can hardly be exaggerated.

Frederic Wood Jones died on 29 September, 1954, at the age of 75, enduring his last illness with the greatest fortitude and courage.

W. E. LE GROS CLARK

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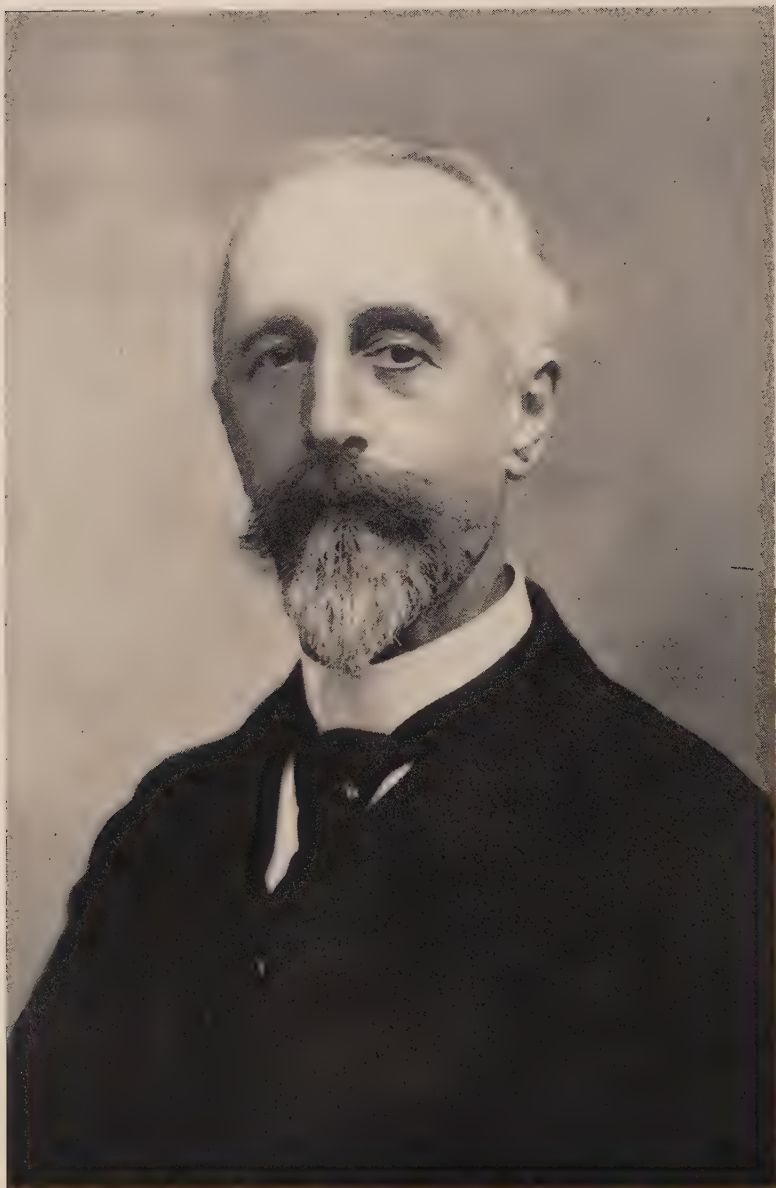
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H. Kayser

HEINRICH KAYSER

1853-1940

A FEW years before his death, HEINRICH KAYSER was persuaded by some of his friends to write down his memoirs. He prepared a manuscript of 342 pages which was never published. The present history of his life is based largely on this autobiography.

Heinrich Kayser was born at Bingen on the Rhine on 16 March 1853. His great-grandfather, Johann Jacob Kayser, coming from peasant stock in East Prussia, was the first to change to an academic profession. He was parson, land surveyor, and philosopher who applied unsuccessfully for the professorship at the University of Königsberg which was later occupied by Immanuel Kant. Kayser's grandfather, August Immanuel Kayser, was a prominent lawyer in Königsberg who acquired a large feudal estate (Friedrichsberg). His father, Johann Jacob Heinrich Kayser, was prevented from completing his studies of law by a serious disease of his eyes and took over the estate of his father, spending much of his time travelling all over Europe. Kayser's mother, Dorothea Amélia von Metz, was the daughter of a Russian army officer, a refugee from the French revolution. The parents were married in Moscow (1843) and after a few years at Königsberg moved to Bingen where Heinrich Kayser was born as the youngest of a family of five. Since the family moved several times, Kayser's education in his younger years was somewhat irregular. During several years his father gave him instruction in Latin, Greek, mathematics and history. He finished his schooling at the Sophiengymnasium in Berlin in 1872.

Kayser began his studies of physics at the then new University of Strassbourg in the spring of 1873. Soon he came in contact with Kundt, the Professor of Physics there. Even though Kayser was only a first year student, Kundt gave him the run of the laboratory and let him work with any of the available apparatus he pleased. The many informal discussions with Kundt gave him a lifelong admiration for this able teacher and scientist. Kundt was what one might call an extreme experimentalist. He tried to discourage Kayser from taking calculus and other mathematical courses. He once told Kayser, 'a physicist must be able to saw with a file and to file with a saw'.

Kayser's studies in Strassbourg were interrupted after the first semester first by the estrangement between his father and the rest of the family and later by a serious attack of pleurisy. After recovering from it he had a leisurely semester at Munich and then returned to Strassbourg where he started on his first piece of research work 'Determination of the ratio of the

specific heats of air at constant pressure and at constant volume from the velocity of propagation of sound', which was published in the *Annalen der Physik* in 1877.

When early in 1877 his father withdrew his further support, Kayser was unable to proceed to his Ph.D. degree under Kundt. He went to Berlin and, supported by his sisters, he took his Ph.D. under Helmholtz with a thesis on the effect of sound intensity on its velocity of propagation, which was completed at the end of 1878. He passed the examination '*cum laude*' in February 1879.

In March 1878, Helmholtz had offered Kayser the position of third assistant at a salary of 90 marks per month. Kayser very gladly accepted this position even though it meant considerable delay in the completion of his Ph.D. work.

Helmholtz, according to Kayser, was by no means as inspiring a teacher as Kundt. His advice to his students was often cryptic and poorly explained even though, in the long run, it always turned out to be sound. However, Kayser had nothing but sincere admiration for Helmholtz's fine personality saying that he never learned to know another man of such high purpose and of such noble attitude of mind.

Kayser's admission to the faculty (Habilitation) as a lecturer (Privatdozent) followed unusually closely on his obtaining the Ph.D. degree, in July of the same year (1879).

In the fall of 1880, the first assistant, Giese, left and Kayser moved up to be second assistant, the first being E. Hagen (later chief of the technical division of the Physikalisch-Technische Reichsanstalt). The position of third assistant was given to H. Hertz. Later, when Hagen left, Kayser moved up to be first assistant and Lummer became third assistant. Kayser was on friendly terms with both Lummer and Hertz, but in his memoirs he complains about the lack of good manners in the former and about the idiosyncrasies of the latter.

In the summer of 1885, Kayser was offered the chair of physics at the Technische Hochschule at Hannover. He took over his duties there in the fall of the same year and stayed there until 1894 when he became successor of H. Hertz in Bonn. His assistant in Hannover was first Kurlbaum and later Paschen.

The offer of the Bonn chair to him is ascribed to the special wish of H. Hertz on his deathbed and was obtained by the University against the opposition of the Prussian government.

The laboratory facilities in Bonn just as in Hannover were not of the best and only after considerable effort was Kayser successful in obtaining a new building which was completed in 1913. When Kayser came to Bonn, Lenard was first assistant, but he left for Breslau before Kayser started lecturing. His assistants were successively Hagenbach, Pflüger, Bucherer, Konen, who was to become his successor, and Eversheim.

In 1887, two years after going to Hannover, Kayser married the widow of

a Berlin merchant, Prümm. She was thirteen years his senior and had five children from her first marriage. He says in his memoirs of her and his mother, 'most of the good and beautiful things that I have learned, most of the happiness that I have experienced I owe to two women, my mother and my wife'. After a singularly happy marriage, his wife died in April 1915.

Kayser retired from his chair in Bonn in May 1920. He lived for twenty years in retirement in Bonn, a lonely man. He died on 14 October 1940.

These are the external facts of Kayser's life. It is fitting to proceed now to a brief discussion of his scientific work. His early work in Strassbourg under Kundt and in Berlin under Helmholtz was in acoustics: An early c_p/c_v determination from the velocity of sound in air, an investigation of the dependence of the velocity of sound on sound intensity and an examination of the influence of temperature on the pitch of tuning forks. He then turned to the phenomena of condensation of gases on solids now known as adsorption. It was Kayser who introduced this designation at the suggestion of Dubois-Reymond. Bunsen, who had carried out similar adsorption experiments with very different results, questioned Kayser's results, but Kayser pointed out that Bunsen's glass fibres were not properly cleaned, which, later on, Bunsen very reluctantly had to admit.

In 1880 Kayser took a course in photography (then still quite new) from H. W. Vogel at the Berlin Technische Hochschule. Kayser learned to make his own plates and films and laid the foundation for a mastery of photography which stood him in good stead in his later work.

Kayser's interest in spectroscopy was first aroused by a series of three lectures of his colleague, E. Hagen, at Christmas, 1881. Kayser recognized immediately that the study of the spectra of different atoms and molecules would eventually lead to information about atomic and molecular structure and therefore he decided forthwith 'to turn all his energy to this field'. He spent every free moment in 1882 on a study of the literature of the subject and, as a result, wrote a small book *Introduction to spectral analysis*, which was published in April 1883. This book was generally well received except for a review by H. G. Vogel, then director of the Potsdam Astrophysical Observatory, who particularly objected to the idea of Kayser that photography would be of extreme importance for spectroscopy. A rebuttal by Kayser was not accepted by the journal (*Zeitschrift der astron. Ges.*) in which the review had appeared.

When Kayser was in the process of preparing for his first original work in spectroscopy, Rowland's famous paper on the concave grating appeared (1883). Rowland had previously worked in Helmholtz's laboratory and, at Kayser's request, sent a grating as a gift. With it Kayser took his first spectra. However, the mounting was in a most unsuitable location and the spectra were not satisfactory. Only after moving to Hannover did Kayser succeed in building a satisfactory grating spectrograph in a suitable location. At this time, accurate wavelength measurements were badly needed. Even some

prominent lines of various elements were known to only several ångströms. Kayser recognized that the iron spectrum would be particularly suitable for wave-length standards and he carried out the first determinations of the wave-lengths. Subsequently, he used these standards for a measurement of the spectra of a long series of elements.

Soon after Kayser had gone to Hannover, the mathematician C. Runge joined the faculty there. He became interested in spectroscopy and joined forces with Kayser in the study of the regularities in the spectra which Kayser obtained. Thus originated the well-known papers by Kayser & Runge on series in line spectra which formed the basis for all later developments in atomic spectroscopy. It is interesting to learn from Kayser's memoirs that all this early work was carried out without a proper comparator, a cathetometer was used as such, and without a calculating machine.

Great as was the importance of this work, Kayser & Runge did miss the real clue to the understanding of the line series in terms of atomic structure, as Kayser regretfully records. This was first supplied by Rydberg's brilliant idea of representing the frequency of a spectral line as a difference of two terms.

After taking over in Bonn, Kayser continued his survey of the spectroscopic literature particularly since it took considerable time to establish spectroscopic equipment in a building that was really not suited for it. He proposed to collect all this material in the form of a handbook of several volumes. Since he felt somewhat out of touch with one branch of spectroscopy, astrophysical spectroscopy, he decided to pay a visit to the large observatories in the United States. There he met Langley, Abbott, Pickering, Hale, Wadsworth, Michelson and Rowland. Upon his return from this trip, he began writing (1897) and finished the first volume in the spring of 1899. The remaining five volumes followed in remarkably quick succession, the last one appearing in 1912. This monumental work he completed all by himself. It was made possible by Kayser's extraordinary memory for detail, and by an exceptional facility in writing. As Kayser reports in his memoirs, he rarely had to change anything in the first draft.

The effect of Kayser's handbook on spectroscopy can hardly be exaggerated. The whole of the earlier literature had here been collected in one place and its value had been assessed, gaps had been pointed out. The rapid upswing of spectroscopy after 1910 stimulated by the work of Rydberg, Bohr and Sommerfeld was made possible by the foundation supplied by Kayser's handbook. But the very rise which it made possible also caused it to become in certain parts rapidly out of date. Anyway, the whole edition was nearly sold out a few years after the last volume was published. Therefore, in 1914, it was decided to prepare a new edition. Some 150 pages of the new first volume were already printed when the war broke out and put an end to this project. Only in 1920, that is, after Kayser's retirement from the chair in Bonn, was it possible to consider what to do about the handbook. By this time the literature had increased so much that it did not seem feasible to have a

completely new edition. Rather, Kayser decided to add two volumes bringing up to date the material on the individual elements in the old volumes 5 and 6. He persuaded his successor, Konen, to participate in this undertaking. However, this collaboration did not turn out too well since Konen was engaged in many other activities and was unable to devote sufficient time to the handbook. While Kayser had his part of the work ready in 1923 for all the elements, Konen's part was never completed and only two sections of volume 7 and one of volume 8 appeared in 1923, 1930 and 1932 bringing the elements to Ir (in alphabetical order) up to date. Kayser felt considerable bitterness about this 'beheading' (köpfen) of his life's work which was increased by his realization that the sections that were published did not live up to the previous standards, since Konen employed the help of apparently rather inexperienced Ph.D. students in his part of the work.

In spite of the intensive work on his handbook during the years 1897 to 1912, Kayser did not neglect his own original work in spectroscopy. Particularly notable during this period is his work on the platinum elements and on the rare earths. In addition, there were a large number of Ph.D. students to look after. More than a hundred took their degree under him in Bonn. More and more young students and spectroscopists from abroad came to Bonn to work under Kayser. It was he, for example, who suggested to A. S. King that he should work on the influence of temperature on the spectrum, a field in which King later made such notable contributions.

Also during this period plans had to be made for the new Physics Building which was opened in 1913. Finally, during this same period Kayser took a very active part in the work of the International Solar Union. He took over the presidency of the commission on wave-lengths whose work has contributed so greatly to the establishment of standards throughout the accessible spectral region. The Solar Union met in Bonn in 1913 when Kayser, in spite of his wife's illness, was a genial host to about 100 people.

Two other contributions which Kayser made in his retirement when he was over 70 years old must be mentioned. His *Tabelle der Schwingungszahlen* and *Tabelle der Hauptlinien der Linienspektren aller Elemente*. These tables have been two of the most useful books for every spectroscopist ever since their publication in 1925 and 1926.

Kayser was not a narrow specialist. His interests were, in fact, unusually wide. While a student in Strassbourg he took a course in hieroglyphics from the famous egyptologist, Dümichen, and became so proficient in it that Dümichen tried to persuade him to give up physics in favour of egyptology. Moreover, he took up Persian in order to be able to read Firdousi and Hafiz in the original. While reconalescing from his attack of pleurisy, he took up wood carving and did so well that his teacher tried to persuade him to become a sculptor.

Kayser had a real passion for travelling. Half his memoirs are devoted to a description of his many trips. During his long life almost every vacation time (spring and fall on the continental system of semesters) he spent wholly

away from Bonn, away from spectroscopy, in the Alps, in Italy, Greece, Egypt, etc. He made a real study of classical art treasures, he knew every room and every picture in Pompeii, he spent weeks in the many museums in Rome, Athens and many other places. He edited and prepared for publication some of the writings of the art historian, Justi, after the latter's death.

Kayser was an excellent linguist. He spoke Italian, French and English fluently in addition to his sound knowledge of Latin and Greek, and in his old age he learned Spanish in order to read Don Quixote in the original language. In addition, he resumed his study of Persian begun 50 years earlier. He spent much of his leisure to read and re-read Homer, Pindar, Aeschylus, Sophocles, Plato, Xenophon and other classical authors.

Kayser visited Great Britain on many occasions and made friends with many British physicists: Schuster, Hartley, Liveing, Dewar, Ramsay, Baly, Lockyer, Fowler, Huggins, Lord Rayleigh and others. He was made an Honorary Member of the Royal Institution at the centenary celebration in 1899. He was elected a Foreign Member of the Royal Society in 1909. He represented the University of Bonn at the 250th Anniversary of the Royal Society in 1912 and, in the same year, on the occasion of the British Association meeting in Dundee, received an LL.D. from St Andrews. He returned once to the U.S.A. for the meeting of the Solar Union in Pasadena in 1910.

Kayser lived a long and rich life. With him one of the founders of spectroscopy has passed away. Since I did not know him personally, I may be permitted to close this history of his life with a quotation from the obituary written by a student and friend of Kayser, Henry Crew:*

'In this German scholar were combined the pioneer and the aristocrat. Into any company which he entered he brought good cheer, for his kindly humour was unfailing. Upon his departure something rare and fine was lost to the world.'

I should like to acknowledge gratefully the kindness of Dr W. F. Meggers in making available a microfilm copy of Kayser's memoirs and the help of Professor W. E. Curtis, F.R.S. in supplying additional material.

G. HERZBERG

* H. Crew, *Astrophys. J.* **94**, 5 (1941).

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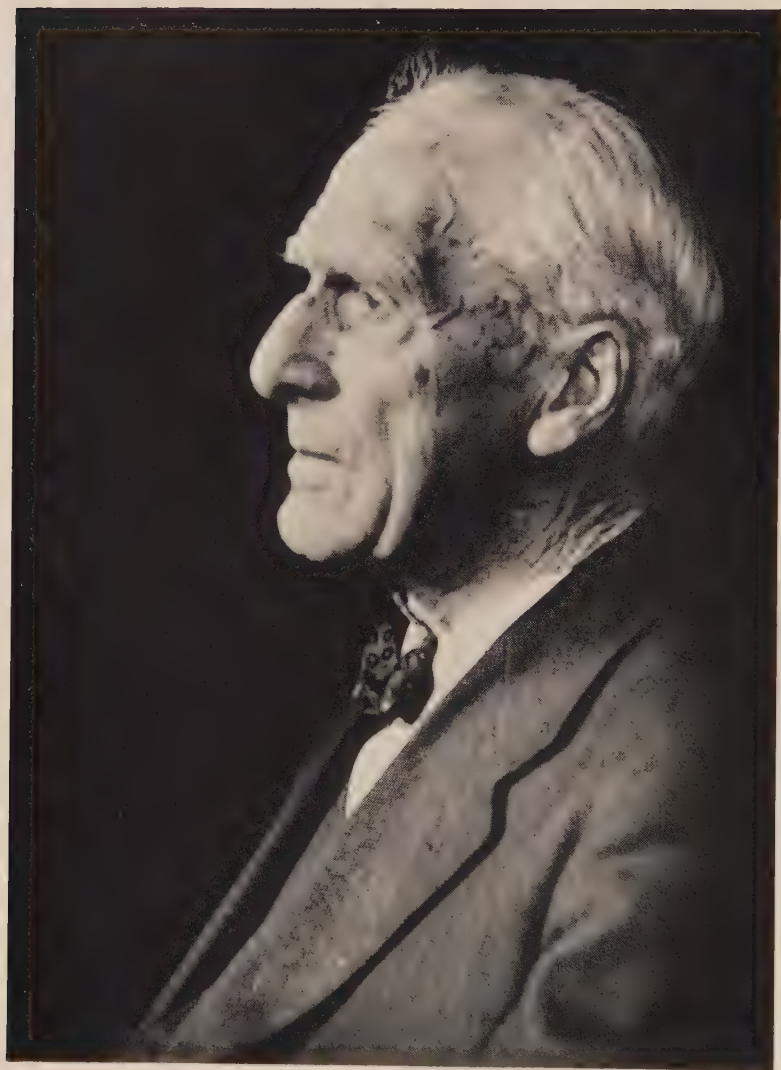
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Arthur Keith

ARTHUR KEITH

1866-1955

THE death of SIR ARTHUR KEITH on 7 January 1955, brought to an end an unusually active and varied career in anatomy and anthropology which had extended over more than sixty years. Of these, a quarter of a century was spent in the Royal College of Surgeons where Keith occupied with great distinction the position of Conservator of the Hunterian Museum.

Arthur Keith was born on 5 February 1866 at Old Machar in Aberdeenshire, the son of a farmer, John Keith, and the sixth of his ten children. At the age of sixteen he left school with the intention of taking up farming, but a young undergraduate of Aberdeen University who came to lodge at the farm inspired him with the idea of an academic career. Accordingly, his father sent him to Gordon's College in order to get grounded in Latin and Greek, and in 1884 he entered Marischal College as a medical student. It was here that Keith came under the influence of James Trail the botanist and John Struthers the anatomist, both of whom evidently provided a powerful inspiration which focused his attention on a scientific career. At the end of his first year he won a prize 'for the highest distinction in the junior division of the anatomy class'—the prize being a copy of the *Origin of species*. He was also awarded a scholarship of which he was very badly in need at the time, for his father's death shortly before had imposed a severe financial strain on his family.

After qualification Keith spent a few months in general practice. Then, in April 1889, he received a letter from Professor Trail offering him a post as medical officer to a mining company in Siam. Trail had been asked by Sir Joseph Hooker (then Director of Kew Gardens) if he knew of a suitable man for this post, who in addition to his medical ability would be capable of making botanical collections. Keith himself was enthusiastic at this opportunity for field studies in the tropical jungles of the Far East, and accepted the appointment without hesitation. He spent three years in Siam, and during this time his medical duties, though often very onerous, gave him enough leisure to collect about 500 specimens of the local flora. This material was later used by H. N. Ridley, Director of the Botanical Gardens in Singapore, in his comprehensive work on the *Flora of the Malay Peninsula*. Having completed this collection, Keith felt that he had fulfilled his botanical commitments and began to pursue scientific studies which were of more direct personal interest to him—an investigation of the anatomy of the local gibbons and monkeys. It was during these investigations that he first decided on a

career in anatomy, and to that end he made up his mind that, at the termination of his appointment in Siam, he would return home to qualify himself for an academic post by taking the degree of M.D. and the diploma of the Fellowship of the Royal College of Surgeons. Meanwhile he continued to assemble anatomical data in the field and published his first paper in 1891 in the *Journal of the Royal Asiatic Society* under the title 'Anatomical notes on Malay apes'. He also published in the same journal geographical notes on some of the Siamese provinces, and an account of a difficult and arduous journey which he made across the Malay Peninsula from Koh Lak to Mergui. Following on this expedition, Keith became seriously ill with a malarial infection, and partly because of his ill health he left Malaya in March 1892 before the termination of his contract with the mining company for whom he was working. After a brief visit to Hongkong and Australia, he reached London in October, with £450 of savings and a firm resolve to seek appointment as an anatomist.

In 1894 Keith was awarded the degree of M.D. of Aberdeen University for a thesis on the myology of the catarrhine Primates, and in the same year he was a successful candidate in the examination for the Fellowship of the Royal College of Surgeons. By this time his savings were almost exhausted and he was still awaiting an opportunity of an academic post. However, by borrowing from his old teacher, Professor Trail, he managed to maintain himself in circumstances of rather severe austerity, but he used the interval of waiting to great advantage by continuing his studies of the anatomy of monkeys and apes. In 1894 he had published in the *Journal of Anatomy and Physiology* (as it was then called) an extensive account of the comparative anatomy of the ligaments of man and apes, and also a paper on the variations of the flexor digitorum profundus muscle. The latter is worth noting for, although only a short paper of four pages, it was based on the author's own dissections of over fifty specimens of apes and monkeys and thus serves to illustrate the extraordinary thoroughness with which he pursued these early investigations. The same comment might be made on his paper on the growth of the brain in man and lower Primates which appeared in 1895. Keith also records that, at this time, he made a most detailed study of over two hundred skulls of anthropoid apes in the collections of the British Museum and the Museum of the Royal College of Surgeons, filling twenty large note books with his measurements and observations. He never published these records *in extenso*, but their compilation provided a very solid basis for his later researches into problems of evolution of man and apes and their inter-relationships (and incidentally impressed him with the remarkable range of structural variability among the higher Primates in individuals of the same species).

In 1895, when his straitened circumstances were beginning to make him feel really desperate, Keith was overjoyed to learn of his appointment to a lectureship in anatomy at the London Hospital Medical School. Thus began his academic career. During the next few years he was much occupied with routine teaching and his scientific contributions were for a time relatively

few and trivial. He revised Treves's *Surgical applied anatomy* in 1901, and continued to edit successive issues of this well-known manual until it reached its seventh edition in 1918. In 1902 there appeared his text-book *Human embryology and morphology*, a book which had immediate success and of which the sixth edition (prepared by himself) appeared as recently as 1948. It was a remarkable book to be written by one who was not, in the usual sense of the word, an 'embryologist'. But, though Keith himself had never carried out any systematic embryological studies, he had a great facility for assimilating and interpreting the work of others, and he had for some time been interested in the embryological basis of congenital malformations. He applied his teratological knowledge very effectively in his text-book, and he also sought to interpret human embryology in terms of comparative anatomy and evolution. It was undoubtedly this somewhat novel approach which made his book so much more stimulating to students (and others) than the more academic and orthodox text-books of descriptive human embryology; since its first appearance it has continued to be widely used by medical students in this country, particularly by those preparing for higher examinations.

At the London Hospital Keith came into close contact with Leonard Hill who was his colleague as Lecturer in Physiology, and some of his most important studies developed out of their mutual interest in problems of respiratory and cardiac functions. He studied the mechanism of respiratory movements in man from a consideration of the anatomical features of the diaphragm and thoracic musculature, combined with an analysis of the joint actions of the sternum and ribs and radiographic observations. As the result of this work he was able to demonstrate the differential expansion of the various components of the lungs in inspiratory movements, and to suggest a relationship to the selective vulnerability of different parts of the lungs to infectious processes. He also applied his observations to a comparison of the relative merits of different methods of artificial respiration. His work on the mechanism of respiration in man was published in a series of papers, and was summarized in an outstanding contribution to *Recent advances in physiology* which appeared in 1909 under Leonard Hill's editorship. Keith had also pursued his studies of respiratory movements into the field of comparative anatomy, and discussed the fundamental nature of the mammalian diaphragm and pleural cavities in an illuminating paper published in the *Journal of Anatomy and Physiology* in 1905. This led him on to a consideration of the mechanism underlying the clinical condition of visceroptosis, and he advanced the idea that this condition might be due (at least in part) to a defective action of what he called the 'diaphragmatic pump'. However, this interpretation was later shown to be of less importance than he supposed at the time.

Of Keith's earlier work, that dealing with the anatomy of the heart certainly won him his greatest distinction. For some time he had been interested in cardiac malformations and their relation to defects of embryological development and to cardiac dysfunction. His comparative studies enabled

him to define more certainly than hitherto the fundamental functional components of the mammalian heart (and, in particular, of the human heart), and it was then that he made the acquaintance of James Mackenzie who had recently designed his polygraph for recording the jugular pulse. At Mackenzie's suggestion, Keith began to concern himself with the anatomical basis of cardiac arrhythmias. Together with Martin Flack (a young Oxford medical graduate who later became a demonstrator in physiology at the London Hospital) he carried out a systematic investigation by macroscopic and microscopic examination of the conducting system of the heart in a large number of mammalian species. It was in the course of this investigation that they discovered a localized mass of muscle fibres of unusual type, mingled with some nerve fibres and embedded in dense epicardial tissue, close to the junction of the right precaval vein with the right atrium. This structure they termed the 'sino-auricular node'. These observations, which were of the very first importance for physiologists in their search for the factors initiating and controlling the cardiac rhythm, were published in 1907, and the identity of the sino-auricular node with the 'pace-maker' of the heart was subsequently established by experimental investigations into the origin of the contractile wave of the heart (particularly by Thomas Lewis in 1910). Meanwhile, Keith maintained his interest in congenital malformations of the heart and conducted a systematic survey of a large number of specimens which had been handed to him by James Mackenzie. This led to further studies on the structures concerned in the production of the jugular pulse, the results of which were published in 1908 and also provided the basis for a series of Hunterian Lectures at the Royal College of Surgeons in 1909. It was undoubtedly this work on the heart—so fruitful in its clinical and physiological implications, as well as the high reputation which he had already established as a human and comparative anatomist, which led to Keith's appointment as Conservator of the Hunterian Museum at the Royal College of Surgeons in 1908.

Arthur Keith's name will always be linked with the Museum of the Royal College of Surgeons, just as, in his time, a reference to the College immediately called to mind the name of Arthur Keith. His distinguished predecessors were essentially academic zoologists and their interests scarcely encroached on the clinical field. Consequently the Museum had come to be looked upon as an appendage rather than an integral part of the College—an appendage which was also felt to be a financial encumbrance. But, with his clinical interests and experience, Keith made it quite otherwise, for under his inspired direction it came to be accepted as one of the finest records of the structure and history of the human body, with particular reference to the anatomical and embryological basis of the surgical disabilities and disorders which may affect it. He interpreted many of these disorders in terms of evolutionary maladjustments or defects of embryological development, and it is undoubtedly true that, in focusing attention on these factors, he provided an inspiration of considerable significance for the development of surgical thought in his day. Under his conservatorship the Museum became an asset

instead of an encumbrance to the College of Surgeons, and during his term of office the scientific prestige of the College was raised to a very high level. One of Keith's main duties at the College was to conduct courses of lectures and frequent demonstrations, and he rapidly acquired a high reputation as a lecturer. Although by ordinary standards his manner of delivery was not particularly good—as a speaker he was somewhat halting and hesitant—he had a capacity for making such intimate contact with his audience that each member almost felt that the lecturer was discussing problems with himself personally. Soon after assuming his new office, he began to give his attention much more actively to problems of human evolution and the diversification of the modern races of mankind. His interest in this field of study had stemmed from the inspiration of his old teacher, John Struthers, who had himself been an ardent exponent of evolutionary processes. Keith thus came within a few years to be recognized as one of the foremost authorities on fossil man. He had made his first contribution to this subject as far back as 1895, when he published in *Science Progress* a critical review of the evidence of *Pithecanthropus* (whose remains had just been discovered in Java by Eugene Dubois). From time to time during his term of office at the London Hospital, also, he had published occasional observations on the comparative anatomy and variations of the skull and skeleton of man and the anthropoid apes. He now returned to this field of study with renewed energy and in 1910 contributed an important paper on certain of the age-changes which occur in the ape skull, including a description of a new type of craniometer which he had devised in order to record some basic cranial measurements not obtainable with the more usual design of apparatus. His observations on the age-changes, particularly those related to the progressive growth of muscular crests and the backward displacement of the foramen magnum on the cranial base, made it clear to him that certain of the contrasting features of adult human and adult ape skulls are explicable in terms of paedomorphosis (though this term did not come into use until twelve years later). Other papers dealt with human skeletal remains from various sites in the British Isles and elsewhere, some of which at that time were presumed to be geologically very ancient, and of special note is the description which he gave of the teeth of Mousterian man which had recently been discovered in a cave in St Brelade's Bay, Jersey. These palaeo-anthropological studies were accompanied by studies of certain pathological conditions which affect the skeletal system in modern man, e.g. acromegaly and progeria, in the course of which Keith suggested that, among other things, hormonal factors might contribute to the development of some of the peculiarities of the skeleton to be seen in palaeolithic types of man. However, he never pursued this interesting proposition beyond the analogical level.

It was during his early years at the College of Surgeons that Keith developed the thesis that modern types of mankind, i.e. *Homo sapiens*, had a much greater antiquity than generally supposed, and he expounded his arguments in favour of this thesis in a series of Hunterian lectures, as well as in a small

book, *Ancient types of man* (1911). His conclusions have since turned out to be partly correct (in the sense that the species *Homo sapiens* certainly has quite a respectable antiquity), though many of the items of evidence on which he then relied have proved to be faulty. For example, the famous Galley Hill skeleton has been shown in recent years by fluorine analysis not to be indigenous to the gravels of the 100 foot Thames terrace in which it was found, but a relatively recent interment. Again, the Ipswich skeleton which was exposed beneath a stratum of boulder-clay by Reid Moir, and assumed to be of preglacial date, was likewise shown later to be of no great antiquity. In 1912 the famous Piltdown skull was for the first time given publicity at a meeting of the Geological Society, when it was described by Smith Woodward as a very remarkable type of early hominid to which he gave the name *Eoanthropus dawsoni*, while Elliot Smith gave an account of the endocranial cast. This was most unfortunately the occasion for a serious difference between Keith on the one side and Elliot Smith and Smith Woodward on the other, a difference which led to a somewhat unhappy estrangement. It is never pleasant to recall scientific disputes of a personal nature (particularly when this involves the necessity of siding with one or other of the disputants), but it is only fair to the memory of Arthur Keith to mention briefly the relevant facts. In his initial reconstruction of the Piltdown cranium, Smith Woodward made rather a curious error, though it was no doubt excusable for one not conversant with the finer details of human osteology. He mistook a paramedian ridge in the obelionic region of the skull for a *median* ridge, and this led him to tilt the larger parietal fragment much too far over towards the opposite side. He thus finished up with a skull of distinctly simian appearance and having an unusually small cranial capacity. A cast of the cavity of the reconstructed skull was given to Elliot Smith to study, as a result of which he published the statement that Piltdown man was possessed of the most simian and primitive human brain ever recorded. Keith detected Smith Woodward's error and demonstrated that the Piltdown skull, when correctly restored, showed no significant difference in its general proportions and endocranial characters from the skull of a modern man. Despite this, in the somewhat animated discussions which followed Elliot Smith was still able to convince himself that the skull and brain both showed markedly primitive and simian characters, and he seems also to have carried conviction to biologists in general (very few of whom, naturally, had any real knowledge of the anatomical details under discussion). On the other hand, Keith was reluctant to continue what threatened to become an unseemly wrangle between scientists and did not press his quite sound arguments to their logical conclusion. If the initial error in the reconstruction of the skull and the interpretation of the endocranial cast had from the very first been freely admitted by all those concerned, it seems not unlikely that the remarkable incongruity between the cranium and jaw might at that time have aroused suspicions about the authenticity of the discovery. Keith himself never ceased to regard the whole question of 'Piltdown man' as something of an enigma,

even though he did accept the majority opinion among anatomists and palaeontologists in this country that the cranium and jaw belonged to the same individual—an early type of man.

One of the interesting sequels of the Piltdown controversy was Keith's presidential address to the Royal Anthropological Institute in 1914. So confident was he of the reliability of his methods in the reconstruction of the Piltdown skull that he offered to engage in a similar reconstruction from portions of a complete (but to him unknown) modern skull which had been suitably prepared by some of his colleagues. Accordingly, two of his fellow anatomists selected a skull and, after recording its measurements and making a cast, cut out the fragments corresponding to those of the Piltdown skull and, a few weeks before his presidential address, handed them over to Keith. This was a singularly courageous thing for Keith to do—allowing himself to be, as it were, put publicly to the test, for he was not to be apprised of the accuracy (or inaccuracy) of his reconstruction until the actual occasion of his lecture. But he fully justified himself, for his reconstructed skull, except for minor features, proved to be almost identical with the original. Another outcome of the Piltdown discovery was the publication in 1915 of *The antiquity of man*. This was perhaps Keith's most widely read book; it proved to be exceedingly popular to the general reader as well as to anatomists and anthropologists, and a second edition (in two volumes) was issued ten years later. The title was somewhat misleading, for it might be taken to imply a critical assessment of the geological evidence for man's antiquity (that is, along the lines of Lyell's classic published under the same title fifty-two years previously). But Keith had had no geological experience, and no doubt the weakest arguments in his book were just those which purported to be based on geological evidence. The main substance of the book (and by far the most important) was anatomical, and it was here that Keith excelled in his descriptive powers. By the avoidance of technical terminology and by the construction of many effective diagrams, he was able to present to the non-specialist the essential features of fossil human material, and he was able to weave the anatomical facts into a coherent story which seemed to make very good sense. A rather disproportionate part of the book was taken up with a consideration of the Piltdown remains (indeed, in his autobiography Keith records that it had been his original intention to devote the book entirely to Piltdown man), and much attention was also given to the now discredited Galley Hill skeleton. The general theme of the book implied an antiquity for *Homo sapiens* which most authorities to-day would consider to be too extreme, but it was not an unfair assessment of the palaeo-anthropological evidence at that time available.

During the first World War much of Keith's attention was occupied with problems of surgical anatomy related to war injuries, and he was called on to arrange a series of special lecture courses at the Royal College of Surgeons and to prepare topical exhibits in the museum. He published a number of lectures in the *British Medical Journal* (1918) on the anatomical and physio-

logical principles underlying the treatment of injuries to muscles, bones and joints, and several articles in the *British Journal of Surgery* on bone-grafting, processes of infection and repair in limb-bones, and the various types of gunshot injury. Some of his war-time lectures, which gave an historical survey of the development of methods of treatment in war surgery, appeared in book form as *Menders of the maimed* (1919). In spite of the distractions of war, he also found time for other activities. In 1914 he had been elected to the presidency of the Royal Anthropological Institute, a position which he held for four years. By now his contributions to physical anthropology had extended over a wide field, covering studies of modern human races (with special reference to their origin and inter-relationships), enquiries into the factors involved in the development of the abnormal crania of achondroplasia, acrocephaly, etc., and numerous lectures dealing with a variety of problems of human evolution. In 1916 he was invited to give the Christmas juvenile lectures and selected as his title *The human machine which all must work*. By all accounts, Keith made an outstanding success of a subject which, by its very nature, must have been exceedingly difficult to expound to an audience of young people. In 1919 the lectures were published in a book entitled *The engines of the human body*, a second edition of which appeared in 1925.

During the years following the war Keith's interests turned more and more to general themes of medical history and to somewhat speculative considerations of evolutionary processes in relation to the origin of man. It is true that he continued to publish systematic reports on ancient skulls which were discovered from time to time, but these were brief and mainly descriptive. He also made an interesting communication to the Anatomical Society (published in 1919) on the nature of multiple exostoses, in which he sought to explain this pathological condition as a disorder of the osteoclastic action which is responsible for the remodelling of the extremities of normal growing bones. But apart from these items his publications for several years were predominantly made up of general articles in the medical or lay press and reports of lectures in which he enlarged on the conclusions to which his earlier work had led him. The fact is that at this time his reputation as a lecturer and as a popular exponent of anthropological subjects caused all too heavy demands on his time and thus distracted him from the more rigorous work of the laboratory. And, of course, he was always kept busy revising several of his books for new editions. He had been elected to the Royal Society as long ago as 1913, in 1921 he received a knighthood, and from 1918 to 1923 he occupied the position of Fullerian Professor of Physiology at the Royal Institution. It is probably true to say that he was then at the height of his distinguished career, and his election to the presidency of the British Association in 1927 came as no surprise to his colleagues. Keith chose for the title of his presidential address 'Darwin's theory of man's origin as it stands to-day'. It was presented as an affirmation of Darwin's general conclusions on the evolutionary derivation of the Hominidae from an ancestry in common with the anthropoid apes, amplified by reference to the accumulation of com-

parative anatomical and palaeontological evidence since Darwin's time. But, on the whole, it was not very enthusiastically received; it brought forward no really novel evidence or new point of view and it received a certain amount of criticism for its somewhat platitudinous treatment of the subject. On the other hand it naturally drew the fire of anti-evolutionists—a fact which seems to have caused Keith a certain amount of surprise (though, in retrospect, it might surely have been anticipated). It was as a result of this, seemingly, that he permitted himself to be drawn into activities of a journalistic type by writing series of articles in popular journals and newspapers on human evolution, on his personal beliefs, on the future of mankind and so forth. It may seem regrettable that Keith devoted so much of his time to expositions of this sort, but he held firm opinions on these matters and he appears to have felt that he was carrying a mission of what is sometimes termed 'rationalism' into what he supposed to be an unconverted mass of irrationalists. But there was one outstanding result of the meeting of the British Association at Leeds—Keith made an appeal for the preservation of Darwin's home at Downe in Kent, and he received an immediate response from Mr Buckston Browne, a retired surgeon, who offered on behalf of the Association to purchase Down House, and to restore and endow it. Some years later, Buckston Browne (also as the result of Keith's personal influence) gave a large benefaction to the Royal College of Surgeons to convert a farmhouse at Downe into a research institute for studies in experimental surgery. This building came to be known as the Buckston Browne Farm (or Research Institute).

In 1930, Keith had the great distinction of being elected Rector of Aberdeen University, and his rectorial address in the following year was entitled 'The place of prejudice in modern civilization.' It was in this address that he developed a thesis which seems to have formed in his mind under the influence of the first world war, the thesis that the spirit of nationalism to-day is a potent factor in the evolutionary differentiation of human races, and that modern war itself constitutes the selective machinery for promoting this differentiation. Unhappily, he referred to war as 'Nature's pruning hook', a most unfortunate phrase which aroused considerable resentment from many quarters. Many years later (1948) he expanded his views in a book which he called *A new theory of human evolution*. There was, in fact, nothing fundamentally new in the main thesis of this book, with the exception that the author attempted to apply the well-recognized conception of social segregation as a basis for genetic differentiation in small isolated communities to the much larger political groups of modern times which are commonly termed 'nations'. But he failed to recognize that national groupings do not permit the degree of genetic isolation which would be required for physical differentiation by natural selection, nor did he take into account the fact that the instability of such groupings could hardly permit whatever isolating factors there may be to persist for any considerable length of time. He also confused the issue by equating the term 'race' with the term 'nation', to the bewilder-

ment of those who have always regarded the term 'race' in its scientific connotation as a taxonomic term to be equated with 'geographical variety' or 'sub-species'. Such reception as this book received was very critical, and it did not arouse the interest which might have been expected from its title.

In 1933, after a severe illness following a lung infection, Keith retired from the Conservatorship of the Royal College of Surgeons, and, having been given the nominal position of Master of the Buckston Browne Research Institute in Downe, retired to live there. A year later he suffered the loss of his wife, who died as the result of a cerebral haemorrhage. She was the daughter of Tom Gray, the artist, and since their marriage in 1898 she had been a constant source of help and comfort to her husband—whether by assisting him in his work or by caring for him during the rather frequent periods of ill-health which so often seem to have interrupted his work.

Keith now settled down to study the skeletal remains of palaeolithic man which had recently been disinterred from the caves of Mount Carmel in Palestine, and in this work (which occupied his attention for several years) he had the assistance of a young graduate of the University of California, Dr T. D. McCown. The results of the investigation were published by the Clarendon Press as the second volume of the treatise on *The Stone Age of Mount Carmel* (1939), an impressive volume which ran to over four hundred pages and twenty-nine plates. In some respects this was one of Keith's best works; it was certainly one of the most complete and systematic of his studies. It was carried out with meticulous attention to detail, the conclusions based on the anatomical evidence were carefully reasoned, and the illustrations (mostly line-drawings prepared by the author himself) were beautifully executed and unusually informative. The whole comprises a standard work which is not only important and illuminating for the anatomical specialist, but can also be read with interest and understanding by anthropologists in general. In the succeeding years of his retirement, Keith settled down happily at the Research Farm, writing occasional articles of a general scientific nature, enjoying the companionship of the young research workers who made use of the opportunities there for experimental surgery, and taking an active interest in the upkeep of the farmland and its stock of animals. During the second World War, he stayed on at Downe, in spite of the fact that this part of Kent was exposed to a considerable amount of bombing. Amidst his other activities he wrote a series of essays on human evolution, and it was these essays which were subsequently gathered together and somewhat amplified as the book already mentioned, *A new theory of human evolution*. In his eighty-fourth year his *Autobiography* was published—a personal and rather intimate record of his distinguished career, but a frank record, for it dwells on disappointments and estrangements as well as successes and friendships, and on mistakes in the course of his scientific investigations as well as on the solid achievements to which the latter also led. In his introduction the author affirmed that this was to be his last book, but only a few days before his death he had finished correcting the proofs of yet another,

Darwin revalued. Like some of his earlier books, the title is perhaps somewhat misleading, for it might be taken to imply a revaluation of Darwinism, or at the least a revaluation of Darwin as a scientist. In fact it is neither of these; rather, it is an abbreviated account of Darwin's family life at Downe, written by a man who, having spent most of his own life extending and amplifying the palaeontological and anatomical evidence on which Darwin had based his conclusions regarding the evolutionary origin of man, and having immersed himself for over twenty years in the atmosphere of the countryside in which Darwin lived and worked, felt that he was particularly well qualified to give a vivid and intimate picture of this great man.

Sir Arthur Keith died suddenly on 7 January 1955. He was active up to the end, for only the previous day he had been out in the orchard pruning his fruit trees.

The brief history of the life and career of Sir Arthur Keith which has been outlined can hardly do full justice to the remarkable versatility of this distinguished anatomist. He was, of course, primarily a human anatomist and his knowledge of human anatomy (particularly in relation to its clinical applications) was quite profound. Secondly he was a comparative anatomist having an unrivalled personal acquaintance with the morphology of the higher Primates. Thirdly, he was a physical anthropologist who allowed his interests to stray far beyond the narrow field represented by measuring tape and calipers. In all these fields of enquiry, he showed a strong appreciation of the historical background of his subjects, and from time to time wrote articles of great interest on the contributions made to anatomy and surgery by some of his predecessors. In particular, he was a fervent admirer of John Hunter, the originator of the Hunterian Museum of which he was so proud to be the Conservator, and the destruction of the greater part of this historical collection in the second world war was to him not only a most severe loss, but also a very intimate tragedy. In addition to his accumulated record of factual observations, Keith was fundamentally a man of ideas. Novel interpretations, stimulating hypotheses and fertile suggestions sparkled from his mind in the course of every piece of research work which he undertook, and no doubt this was one of the reasons why his lectures and writings always attracted a wide public. Many of his ideas never came to fruition—for example his suggestion that the movements of the alimentary canal might be initiated and controlled by 'nodal mechanisms' similar to that of the heart, his conception that certain orthopaedic deformities might be explained on an evolutionary basis, and a number of his speculations on the factors which have determined the evolutionary diversification of human varieties. But these were the inevitable outcomes of a restless and enquiring mind, and are only to be properly balanced by viewing them in the perspective of the advancing fields of knowledge in which he took so active a part. The fact is, of course, that Keith was a pioneer in many branches of anatomical study and, while slowly accumulating data himself, he was always eager to seek some coherent interpretation for the data already available even though at that time they

might still be inadequate for reaching firm conclusions. Particularly was this the case in the field of palaeo-anthropology, where the continual accession of additional material compelled him more than once to reverse his original opinions. But Keith was possessed of a sincerity of mind and an intellectual humility which allowed him at once to recognize and admit the validity of new evidence even when it ran counter to previous pronouncements of his own. That some of his interpretations may seem in retrospect to have been premature is no more than a reflection of the rapidity of the advances in knowledge which were being made in his chosen fields of study, advances which at that time necessarily compelled quite frequent revisions of current opinions.

Apart from his claims to distinction as a scientist, Keith was in himself a much loved man, kindly and gentle in manner, friendly and unassuming, and of a somewhat retiring disposition. He liked the simple life, partly no doubt because it suited his rather frail physique, but mainly because the quiet and homeliness of the countryside always gave him a full measure of enjoyment and satisfaction. Many of Arthur Keith's old friends will look back with particular pleasure to the occasional visits which they made to Downe when he had retired there. He was always ready to receive them and to join in happy conversation about things of the present and things of the past. It seemed so entirely fitting that this devoted student of evolution should himself spend the latter part of his long life of evolutionary studies in the countryside where his great predecessor Charles Darwin had once lived.

W. E. LE GROS CLARK

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C. W. Lewis

GEORGE MARTIN LEES ·

1898-1955

GEORGE MARTIN LEES, son of George Murray Lees of Edinburgh, was born at Dundalk, County Louth, Ireland, on 16 April 1898. He was educated at St Andrews College, Dublin (1906-1915) and immediately thereafter attended the Royal Military Academy, Woolwich, in preparation for taking part in the first world war, then in progress. From Woolwich he was commissioned in the Royal Artillery, in which he served in France. He soon transferred to the Royal Flying Corps and won the M.C. There followed a tour of duty as a flying instructor in Egypt, where he gained his first experience of the Middle East, a region on which he was later to become the leading geological authority. From Egypt he moved to Mesopotamia (now Iraq) and saw further active service in air operations, for which he was awarded the D.F.C. He took part in the capture of Kirkuk from the Turks and made a forced landing behind the Turkish lines in what is now the great Kirkuk oilfield, regaining the British lines with difficulty on foot.

After the war Lees remained for a time (1919-1921) in Iraq in the civil administration. He received a testing and perilous assignment as Assistant Political Officer among the turbulent tribes of Southern Kurdistan. In the vain hope of avoiding costly military occupation the British authorities had decided to set up in Kurdistan a buffer state between the Persians and Arabs under a semi-autonomous ruler. Lees was one of a small group of picked officers whose function was to advise the ruler and his administration and organize the training of local armed forces. Unfortunately for the plan, the Kurd ruler betrayed the trust placed in him and soon sought to set himself up as independent king of Kurdistan. Although unpopular as a tyrant, he had a religious following and succeeded in organizing considerable forces for the overthrow of British authority. Lees, in charge of the wild and mountainous Halabja district close to the Persian frontier, was besieged in his house by hundreds of armed men and only escaped with his life by an ingenious ruse and by great courage.

Later he returned when the country was pacified, and helped the process further by his influence and by personal negotiation with one of the most dangerous tribal leaders. An exciting and modest account of these events was given by Lees to the Central Asian Society in 1928 ('Two years in South Kurdistan'). His successor, Major Noel, who came to Halabja as Assistant Political Officer about two years after Lees, stated 'I can testify to the great reputation he left behind him. During the six months I spent there I travelled

extensively, and in the remotest of Kurdish valleys and the smallest of hamlets I was continuously regaled with stories of Lees's exploits. Perhaps what specially appealed to the Kurds was his happy facility for raising a laugh. Lees's stories and the jokes he made are still remembered.* Another associate of those days writes: 'Lees succeeded in impressing his personality to an unusual degree on the whole of his extensive district. A good working knowledge of the language, sound administrative ability, a rigid sense of justice tempered with a sympathetic comprehension of tribal mentality, and many feats of physical prowess all combined to make him something of a legend.†

Lees returned to England on leave in January 1921, and finding himself out of sympathy with policies which were then being put into operation in Kurdistan, he handed in his resignation in April. This was the turning point in his career. He set to work on geology with great industry at the Royal School of Mines and in October of the same year (1921) joined the Anglo-Persian (later Anglo-Iranian) Oil Company as an Assistant Geologist. With this company he remained until ill health forced him to retire in 1953, and he continued as adviser to the company (now become the British Petroleum Company) until his death in 1955.

In 1922 and 1923 Lees was back in the Middle East, and in the winter 1924-1925 he was selected to conduct the eminent Hungarian geologist, Dr Hugo de Böckh, on an oil-finding tour of south-west Persia. The result of this season's work was the recommendation of a number of structures for exploration, of which four (Haft Kel, Gach Saran, Agha Jari and Pazanun) were later drilled and proved to be important producing fields. The theoretical results were offered to the 1928 meeting of the British Association as part of a symposium which was published in book form in 1929 with the title *The Structure of Asia*, edited by Professor J. W. Gregory. The contribution by de Böckh, Lees and F. D. S. Richardson, as co-authors, occupies more than half the book. The editor explained that it 'differs in scope from the rest of the book, as it states the new evidence as well as discussing it. That chapter, owing to its new information, maps and sections, will rank as one of the primary documents on the geology of South-Western Asia.' Here, for the first time, the Iranian Plateau was recognized to be a Median Mass—a term then first introduced into geological literature in the English language—between the orogenic belts of the Elburz and Zagros ranges, and the Zagros were divided into an inner Zone of Nappes and an outer Folded and Sheared Zone, driven towards the Foreland, the rigid tableland of central Arabia. In a later paper (Lees and Richardson 1940) the two main zones of the orogen were renamed the Zone of Overthrusting and the Normally Folded Zone, and the latter was shown to contain some overthrust folds which had first been classed in the Zone of Nappes. Together, these two papers comprise a

* *J. Central Asian Soc.* 1928, 15, 24.

† Cecil J. Edmonds, C.M.G., *The Times*, 3 February 1955; and *J. Roy. Central Asian Soc.* 1955 (obituary).

corpus of information on the stratigraphy and structure, underground as well as at the surface, unparalleled for any part of the Alpid mountain system. There resulted from the work also important contributions to the theory of salt-domes and salt-glaciers (1927, 1931, 1944) as well as oil geology (see bibliography).

The following season, 1925-1926, Lees made with K. Washington Gray a pioneer geological survey of Oman, that anomalous eastern extremity of the Arabian peninsula formed of barren and jagged mountains which rise mysteriously between the sea and the flat, sand-carpeted interior of the country, the Empty Quarter. The resulting papers offered to the Geological Society (1928) and Geographical Society (1928) remain the standard works on the region. Lees showed that the Oman arc is built up of a complex of thrust sheets, folded and thrust at more than one period, and is structurally as great an anomaly upon the otherwise undisturbed Arabian foreland as its rugged ranges, rising to nearly 10 000 ft., are topographically in a country of plains and plateaux. Here we find already a preoccupation with the problem of mountain ranges that strike out into the ocean, which formed an important theme in his last Presidential Address to the Geological Society thirty years later and was illustrated in the frontispiece of that address by a photograph of the Armorican strike-lines at Finistère running out into the sunset across the Atlantic. Ten pages of Lees's early paper (1928) are devoted to a thought-provoking discussion of the question whether the Oman ranges link up with the Sind arc as assumed by such masters as E. Suess, Argand, and Krenkel, or strike out into the Arabian Sea and Indian Ocean, to form part of a meridional orogenic belt as postulated by L. Kober. Lees favoured a compromise solution based on the two periods of folding. He wrote again on this subject ten years later (1938).

So admirable is the grasp of fundamental problems and of Continental literature in three languages displayed in this Oman paper, that it is difficult to realize that the work was done when Lees was aged 27 and published when he was 29; and, moreover, that he had not taken up geology until he was 23. A partial explanation is to be found in the fact that in 1926-1928 he obtained nearly two years' study leave at the University of Vienna, where he attended lectures by F. E. Suess and L. Kober (who five years before had published his well-known book *Der Bau der Erde*) and thus acquired that invaluable asset, familiarity with the German language and literature. The University of Vienna awarded him a Ph.D. for his thesis on the geology of Oman.

After the academic interlude in Vienna the roving life of an oil geologist was resumed. In the years 1928-1930 Lees examined oil prospects and oil-company organization and administration on the geological side in the United States, Canada, Egypt and Germany, and also carried out regional surveys in the Kermanshah province of Persia and in Iraqi Kurdistan. At the end of this tour he submitted the first of a series of reviews of oil prospects in all countries of the world where his company might be able to operate,

through its subsidiary the D'Arcy Exploration Company. The Kermanshah survey was the first of a series, for which it served as a model, and it 'was never surpassed in quality of observation and interpretation.'*

On 1 November 1930 Lees was appointed Chief Geologist of the Anglo-Iranian Oil Company. He was then 32 years old and had been with the company just eleven years. He held this important post nearly twenty-three years, during which time vast expansion took place in the company's activities in many parts of the world. In the decade 1930-1940 most of the principal oil fields in Persia were discovered, also the huge fields at Kirkuk in Iraq and at Kuwait, and the smaller one in the Qatar peninsula in Arabia, where Lees had obtained from the Sheikh a two-year option in 1926. In addition exploration was pushed forward by the Anglo-Iranian Oil Company by surveys of nearly 100 000 square miles in Persia and also in Great Britain, and in association with other companies in New Guinea (Papua), India, Burma, Trinidad, Nigeria, and elsewhere.

Lees did not allow himself to be wholly absorbed by these manifold activities. In 1931 he married Hilda Frances Andrews, of Moseley, who is author and editor, under her maiden name, of various musicological books. She survives him, with their son Robin.

The search for oil in Britain and its discovery in sufficient quantity to pay for the costs of exploration and to be a valuable help during the submarine blockade of 1939-1945, form a fascinating story. The initiative, driving force and responsibility were Lees's. Under the Petroleum Production Act of 1934, which nationalized the country's oil rights, the first prospecting licences were granted in 1935 to the D'Arcy Exploration Company. They covered nearly 7000 square miles in the east and south-east of England and a small area in Scotland, east of the Pentland Hills. Choice of the most likely points for drilling involved detailed surface and subsurface surveys of favourable structures, if possible in the neighbourhood of a known impregnation or seepage. No time was lost, and within ten years of the granting of the concession the D'Arcy Exploration Company had drilled fifty-two deep and forty-three shallow exploratory borings, with a total depth of over 100 miles.†

In the south of England there were disappointments. Lees and his colleagues had discovered oil impregnations in the Wealden, Purbeck and Corallian formations on the Dorset coast in 1933, and even a seepage, between Ringstead and Osmington, from the Corallian Beds. Accordingly structures suitable as possible reservoirs were drilled at Poxwell, Portsdown, Kingsclere, and in the Weald, but no oil was found. The borings nearly all proved much greater thicknesses than had been predicted and this surprised everyone but Lees. No one had foreseen, however, that although thick even on anticlinal axes such as Portsdown and Kingsclere the Jurassic rocks would be developed in unsuitable facies. On the theoretical side of the account, these borings proved the correctness of Lees's brilliant deduction that the Tertiary anticlines

* P. T. Cox, *in lit.*

† *Nature, Lond.* 1945, **155**, 567.

in the Chalk and Jurassics are not 'posthumously' reactivated folds along ancient anticlines in the basement, as was generally believed, but are 'free folds' in a thick sedimentary cover; consequently it would not have been justifiable to ignore them in any serious search for oil in Britain.

In the Midlands pertinacity and geophysical effort were rewarded. The Nottinghamshire oilfield was discovered and brought into production just at a time when total war made every ton of home-produced oil invaluable. The chief producing wells are about 8 miles north-west of Newark at Eakring, Duke's Wood, Caunton and Kelham Hills, and there is a small isolated field at Formby, between Liverpool and Southport. The production in the Nottinghamshire field is from Millstone Grit and, to a lesser extent, from Lower Coal Measures, at depths of 1900 to 2500 ft. The Formby field is a small accumulation in Keuper Waterstones sealed by Glacial clay, at a depth of only 100 to 120 ft. In addition gas fields were found in Eskdale, Yorkshire, and near Dalkeith in Scotland, and the first known British reserves of potash were discovered in the Permian of Eskdale; but these last have so far not proved economical to work, owing to the great depth (3650 to 4775 ft.).

The Nottinghamshire crude oil is of good quality, with good light and lubricating oil fractions and specific gravity from 0.83 to 0.89. Peak production exceeded 100 000 tons in 1942-1944, but it is now about 55 000 tons a year. Up to the present it has totalled nearly 900 000 tons. Only a small pumping jack run by electricity marks the position of each of about 130 wells, and there is no disfigurement of the landscape. The profusion of borings is normal for sand fields where the oil-producing sandstones are partly lenticular and often independent.

As a by-product of this campaign of drilling and geophysical work an immense amount of information was obtained about the underground extension of British coalfields. Thus a double economic benefit was conferred on the nation by the enterprise of the D'Arcy Exploration Company.

In addition to the economic rewards, however, there was an incalculable gain to academic geology in the new knowledge of underground structures and stratigraphy obtained. It would have been in accordance with commercial precedent if all this information had been filed away in the company's offices and treated as secret. But this was not Lees's way. He was himself as much stimulated by the theoretical results as by the main economic objectives, and he desired that they should be thrown open to discussion by the geological world and that the widest scientific use should be made of them. The result was two long papers offered to the Geological Society: one in 1937 with P. T. Cox on 'The geological basis of the present search for oil in Great Britain', another in 1944 (published 1946) with A. H. Taitt on 'The geological results of the search for oilfields in Great Britain'. At the reading of these papers the rooms of the Geological Society were packed to capacity, and it is no exaggeration to say that the material presented was epoch-making and unprecedented in the history of the Society. In opening the discussion at the conclusion of the second paper (6 December 1944) the President,

Professor W. G. Fearnside, F.R.S., said: 'Never before had so much exact and new information about the underground geology of Britain been presented to the Society, and rarely was a synopsis so clearly and concisely expressed', and a series of distinguished speakers after him paid tribute to the enlightened policy which made available so much revolutionary information and to the ability of the authors in analyzing and summarizing it for the benefit of science.

In 1943 the Geological Society acknowledged Lees's leading share in the discovery of oil in Britain and the value of his geological work in this connexion and in Persia and Oman by awarding him the Bigsby Medal. Candidates for this medal must be under the age of 45, and in view of the magnitude of his achievements it is remarkable that he still qualified for it, even though only by a few weeks. Five years later, in 1948, he was elected a Fellow of the Royal Society.

In addition to his ever-expanding duties as Chief Geologist to his company, Lees throughout this period followed up varied geological interests, as indicated by the titles of his papers. Some deal with the problems of salt domes and the problematic age of the saline series in the Punjab Salt Range, others with the floor of the oceans and the meaning of the continental shelves, and one (with N. L. Falcon) gave to the Royal Geographical Society (1952) an account of an anticline in the Mesopotamian plains which has risen 50 ft. in the last 2000 years, disrupting an old irrigation system. During the 1939-1945 war he was also seconded to the Petroleum Division of the Ministry of Fuel and Power, and after the war his knowledge and gifts on committee led to his being appointed a member of the Geological Survey Board and the Colonial Geological Surveys Board and to various committees of the Royal Society as well as on the Council of the Geological Society. Finally, for the two years 1951-1952 he was President of the Geological Society. He was the first geologist practising his profession in industry to have achieved this distinction or to have been elected F.R.S.

The steadily mounting pressure of all these activities eventually overtaxed even Lees's amazing capacity for work. New ventures had been started after the war by his company in Sicily, Switzerland and East Africa, each with its special geological problems, and meanwhile exploration was being extended in Persia, Papua and Nigeria, while new fields were being brought into production in the established Middle East areas. Lees's job, even without outside distractions, was big enough to provide work for a team of geologists. His health gave out in 1952 and after a brief come-back he was obliged to retire in 1953.

To the word 'retirement' Lees gave a special and peculiar meaning which would not be accepted by any lexicographer. He continued as Geological Adviser to the British Petroleum Company, the Burmah Oil Company, the Iraq Petroleum Company, and the Société Nationale des Pétroles d'Aquitaine, and continued to serve on a number of boards and committees, and on the local magistrates' bench. When I visited him at his lovely home, The

White House, Chipping Ongar, in September 1954, only four months before his death, he had recently returned from a professional tour in Aquitaine and was perfecting a cross-section of the Pyrenees, several yards long and drawn, like all Lees's geological sections, to true scale. This section and its implications shared first place in his mind at that moment with sections with which he was experimenting on his drawing table, to explain intricate problems raised by borings on the outer flank of the Jura Mountains.

Lees's last public recognition was the award of the Sidney Powers Memorial Medal, in April 1954, by the American Association of Petroleum Geologists.* This medal is regarded by American oil geologists as the highest distinction they can achieve, and it had never before been awarded to a non-American. He died on 25 January 1955.

An anonymous writer in the Company's magazine† paid this tribute: 'Through all the swelling tide of exploration and development, by virtue of his enormous knowledge, his quickness of apprehension and capacity for constructive thinking, his practical good sense and skill in exposition, and his good humour and reasonableness, he has exercised a unique influence on the counsels of our own and our associated companies. To his staff he has been an incomparably stimulating leader.' Dr C. T. Barber, of the Iraq Petroleum Company, has written: 'He eschewed the frills of oratory, and on the platform was a forceful and lucid exponent both of geology and of the economic principles of the petroleum industry, but it was at the conference table that his dialectic gifts shone most brightly and that his wit enlivened what his intellect illuminated. But to know George Lees and to savour the full flavour of his personality, it was necessary to climb with him the Kurdish mountains or to tramp with him the Purbeck Hills and the Dorset Downs. Then his companions, rarely more than one or two, were not only treated to a brilliant analysis and synthesis of geological structure, but also to a sensitive appreciation of the history and culture of the inhabitants, with whom, be they Kurd or Celt, he was on terms of intimate friendship. To have known George Lees was a privilege, and to work with him an inspiration.'‡

Had Lees not been carried off at the zenith of his powers, there can be no doubt that geological science would have been incalculably enriched by further syntheses and expositions of his mature conclusions. One major work of which we have been robbed is a definitive 'Geology of Persia'. Another, still more important, is a full treatment of his views on the dynamic evolution of the earth: he wrote of this to an American colleague: 'Perhaps some day if I can overcome my mental inertia I may expand my matériel into book form, but it will be a formidable task. . .'.§

Meanwhile we have the two Presidential Addresses to the Geological Society in 1952 and 1953. Each address is complete in itself, but the second

* Citation at St. Louis, by E. De Golyer, *Bull. Amer. Assoc. Petr. Geol.*, **38**, no. 7, July 1954, pp. 1627-1631.

† *B.P. Magazine*, 1953, p. 53.

‡ *Petroleum Times*, 4 February 1955, p. 143.

§ Copy of letter dated 22 August 1954.

follows naturally from the first, to form a single closely knit, ruthlessly logical exposition and argumentation of dynamic geology. Unlike so much writing on this subject, the addresses do not start with a theory or a series of hypotheses and then pick at random the geological observations that support the writer's opinions (conveniently passing over in silence the multitudes of unfavourable facts), but proceed by inductive reasoning from simple folds to cosmology. At every stage they are based on an ample knowledge and an insistence on the geological evidence even if at the sacrifice of preconceived notions and geophysical or cosmological theory. No geologist, geophysicist or cosmologist in future will be able to afford not to read and reflect on these addresses. All will be stimulated and will benefit by the shocks to their complacency and the original turn given at every stage to the interpretation of the facts. One large American oil company had the second address reprinted for issue to all its staff. In London so much interest and controversy were aroused that a joint discussion was arranged between the Geological and Royal Astronomical Societies. This took place on 2 February 1955,* but unhappily the discussion suffered disastrously from the cloud of Lees's death a few days previously and from his absence. He had a catalytic action in geological discussion.

The first address begins with a characteristic happy phrase: 'Much of my professional life has been spent by, with and from anticlines, for the anticline to the oil-geologist is as honey to the bee.' Foreland folds were chosen because of the knowledge of them gained by the drill and their relative simplicity. Examples are described and discussed from all parts of the world and in every geological milieu, and the conclusion is reached that all types, even complex structures such as diapirs and imbric zones, result normally from contraction of the deep underlying crust, not from lateral pressure transmitted through the sedimentary cover in which the structures are expressed. Moreover, the underlying 'rigid' crust has itself always been folded in some previous period, often more than once. The oldest rocks visible at the surface or drilled in any part of the world show unmistakable signs of folding. Therefore compression of the crust has been the dominant theme throughout all ascertainable earth history, although at times and in certain places there may have been subordinate phases of tension.

The second address, 'The evolution of a shrinking earth', starts from this point and follows on to the conclusions, qualitative and quantitative, always holding to the geological evidence and refusing to be confused or side-tracked by geophysical theory. Whenever a geophysical hypothesis or assumption conflicts with geological observation it is given a friendly jolt and some alternative is preferred. A *bête noir* is 'mathematical exhibitionism', which too often misleads the mathematician and the reader by implying 'that a result which can be calculated, almost regardless of the validity of the assumptions made, is preferable to an estimate based on deductions from

* Abstracts in *Nature*, *Lond.* 2 April 1955, 175, 575-6, and *Proc. Geol. Soc., Lond.* no. 1521, 9 March; other discussions also in the house journal of the Standard Oil Company of New Jersey, 1955.

observed geology'. This address contains a masterly review of the structure of the continental nuclei, or shields, of the geological and geophysical evidence so far available for the nature and history of the ocean basins, and of the mechanism and possible causes of mountain-building. The whole is a challenge to the geologist to stand on his own feet, an invigorating call to independent thought and judgment, and above all an exhortation to more and yet more observation.

The geologist who reads this address is constantly stimulated also by the suggestive and fruitful juxtaposition of facts and the unexpected conclusions to which Lees shows them to point. For example, we have all read so often that we have long ago built into our unconscious the concept, expounded especially by able German geologists such as Axel Born, of the continental nuclei growing by the welding on of successive orogenic rings; and we are also aware, though in a different compartment of our brains, that nearly all the orogens embody cores of ancient crystalline rocks essentially similar to those composing the shields. Lees points out that these inlying cores are fatal to the concept of accretion: they show, rather, that the shields were originally larger and that the orogenic rings represent encroachments, not additions. On almost every page there is some such stimulating shock to the reader's preconceptions. The Central Himalayas may be sinking relative to the Ganges plain; the rise of Scandinavia since the Ice Age is probably nothing to do with the melting of the ice cap; currently accepted calculations of the relative densities of the continents and ocean basins 'are based on such insecure premises as to be almost valueless'; such examples could be continued at length.

The reader is rewarded in addition by touches of poetry, which lighten his task and at the same time are an agreeable revelation of the man. Lees felt that it is in the relationships between the continents and the oceans—which hide 70 per cent of the earth's surface from geological observation—that will be found the key to the great geological secrets. After a flight round the world in 1950 he wrote 'These immense oceans have an irresistible fascination, and as the aircraft winged its way above the deep blue expanse bending out of sight beyond the curving horizons, one pondered on their significance and past history.'* In his Address he writes:

'Continental margins have always fascinated me, particularly where a major fold system strikes boldly into the unknown of the oceanic depths. I have sat in contemplation on the Kerry rocks of south-western Ireland and seen the great Hercynian ranges warping downwards through a magnificent fiord phase of drowned valleys into the water of the Atlantic; I have observed with awe and wonder the inspiring view of the Armorican ranges confidently striking into the turbulent Atlantic at the Pointe du Raz, Finistère, and I have seen the great Atlas mountains with their component subdivisions heading strongly seaward. I have seen the Arabian and the Indian coasts, made a complete circuit of Australia and traversed the Pacific: I have seen

* *Geol. Mag.* 1950, 87, 144.

the Californian edge of the American continent and the great drowned valleys of the British Columbian coast; I have seen the bevelled stumps of the one-time mountains of Nova Scotia striking freely into the Atlantic and I have traversed the eastern seaboard of the United States to the point of Florida and the Antilles loop to Trinidad and into Venezuela. All this and more I have seen, and although one's gathered visual impressions are not evidence, they do at least provide a framework for an analysis of this most tantalizing and elusive problem of geohistory.*

It is impossible to summarize here Lees's geological philosophy, but an indication may be given by saying that he believed in a continually shrinking earth, and in a total contraction too great to be accounted for by cooling alone, so that some other factor such as self-compression would have to be invoked; that he disbelieved in the geophysicists' picture of the earth as a layered structure consisting of successive shells of different composition differentiated from an original melt, but preferred the planetismal hypothesis and a layering of physical properties only, perhaps phase changes due to increasing pressure towards the centre; and that he thought the fundamental differences between ocean basins and continents had been exaggerated, and that no oceans were permanent. Of the hypothesis of continental drift he stated: 'I, like so many of us, have been through the earlier enthusiasms for the Wegener hypothesis and have come out at the other side.' On the frontispiece of the Address is a photograph of the moon. In its surface features, so different from anything on earth, Lees saw a likely replica of the earth in its early stages of evolution, before the atmosphere and hydrosphere had obliterated all trace of the original structures formed by infall of meteors.

I am indebted to Mrs Hilda Frances Lees for help in assembling data for the foregoing, and to her and Mr P. T. Cox for critically reading and correcting the typescript. All the dates and facts have been checked in the office of the British Petroleum Company, largely by Miss P. B. Lapworth.

W. J. ARKELL

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* Second Presidential Address, *Quart. J. Geol. Soc.* 1953, **109**, 237.

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J. E. Lemard-Jones

JOHN EDWARD LENNARD-JONES

1894-1954

JOHN LENNARD-JONES was born on 27 October 1894 in Leigh, Lancashire and was educated at Leigh Grammar School, where he specialized in classics. In 1912 he entered Manchester University, changed his subject to mathematics in which he took an honours degree and then an M.Sc. under Professor Lamb, carrying out some research on the theory of sound. In 1915 he joined the Royal Flying Corps, obtained his Wings in 1917 and saw service in France; he also took part in some investigations on aerodynamics with Messrs Boulton and Paul and at the National Physical Laboratory.

In 1919 he returned to the University of Manchester as lecturer in mathematics, took the degree of D.Sc. of that university and continued to work on vibrations in gases, becoming more and more interested in the gas-kinetic aspects of the subject as his paper of 1922 in the *Philosophical Transactions of the Royal Society* shows. In 1922, on the advice of Professor Sydney Chapman, he applied for and was elected to a Senior 1851 Exhibition to enable him to work in Cambridge, where he became a research student at Trinity College and was awarded the degree of Ph.D. in 1924. At Cambridge under the influence of R. H. Fowler he became more and more interested in the forces between atoms and molecules and in the possibility of deducing them from the behaviour of gases. This was the period just before the introduction of quantum mechanics and no method existed by which these forces could be calculated from first principles. Lennard-Jones introduced the empirical form for the potential energy of two molecules at a distance r from each other

$$A/r^n - B/r^m,$$

a form known by his name and which is still frequently of use. It was during the tenure of his 1851 Exhibition at Cambridge, also, that in collaboration with Ingham he calculated the lattice energy of various crystal structures using this form for the potential energy.

In 1925 he was elected Reader in Mathematical Physics in the University of Bristol where Professor A. M. Tyndall was building up a staff for teaching and research in the H. H. Wills Physical Laboratory, at that time under construction. Eighteen months later he was appointed Professor of Theoretical Physics in the same University, a Chair being created for him. At Bristol he continued his researches on interatomic forces, both in gases and in solids. Noteworthy achievements of this period are his explanation of the constants of van der Waals's equation in terms of molecular forces, and his contribu-

tion of a chapter on Interatomic Forces to the first edition of R. H. Fowler's *Statistical mechanics*. At this period, too, quantum mechanics was discovered and applied to the problems of atomic physics; Lennard-Jones studied this subject for a year (1929) at Göttingen at a period when both Pauli and Heisenberg were there, and introduced the new theories to the Bristol group. He based his own researches more and more on quantum mechanics, applying them to problems both of molecular structure and of the solid state. The tradition of the Bristol laboratory for work in the solid state was laid during this time; its potential importance was appreciated by the Department of Scientific and Industrial Research, and Lennard-Jones obtained a grant from that Department to enable him to employ an assistant to work in this field. Harry Jones, now Professor at Imperial College, was appointed and his fruitful work on metals was started under Lennard-Jones. It was in this period, too, that Lennard-Jones began his well-known work on the theory of molecular orbitals in theoretical chemistry, of which he is one of the founders. Indeed the method of molecular orbitals was for some years known as the 'Hund-Mulliken-Lennard-Jones method'. In 1929 he used the theory in a paper entitled 'The electronic structure of diatomic molecules' to give the first explanation of the paramagnetism of the oxygen molecule, an important pioneering contribution which proved to be the starting point of many later developments carried through in the United States and elsewhere.

In the light of its importance to-day, it is interesting to recall the status in this country of theoretical physics in 1925 when Lennard-Jones was appointed at Bristol. The idea that a man of mathematical training should hold a position in a department of physics was a new one, and research in theoretical physics had in the main been carried out by men attached to departments of mathematics. Lennard-Jones was admirably equipped to prove the value of an appointment of this kind. He had quite exceptional powers of exposition; in his hands the new and strange ideas of quantum mechanics assumed an almost illusory simplicity; and he was extremely interested in experimental science and its relation with theory.

Two years after his first appointment the H. H. Wills Physical Laboratory was completed, the year in which his Chair was established. He subsequently took a leading part in securing substantial endowments for the laboratory, both from Mr Melville Wills and from the Rockefeller Foundation. The Chair which he held received a permanent endowment as a result of these generous gifts and was named the Melville Wills Chair of Theoretical Physics. Moreover these and earlier endowments made possible the creation of research fellowships, some to be held by physicists from outside England, both in theoretical and in experimental physics. The two first appointments were Herzberg and Delbrück, both of whom worked in Lennard-Jones's field of interest.

At Bristol too he showed his ability for administration and his interest in it, taking a full share in the making of policy within the University and for two years acting as Dean of the Faculty of Science.

In 1932 he was elected to the Plummer Chair of Theoretical Chemistry in the University of Cambridge, and at the same time accepted an invitation to become a Professorial Fellow of Corpus Christi College. If in his Bristol appointment he was the first man in this country to hold a Chair of Theoretical Physics within a physics department, certainly he was the first man to hold a Chair of Theoretical Chemistry anywhere in the world. In Cambridge he set out to build up a school of theoretical chemistry in which the concepts of quantum mechanics and of interatomic forces were to be applied to a wide field of phenomena in both physical and organic chemistry. In this he was extremely successful. His pupils include some of those who are now leaders of the subject such as Coulson, Devonshire and Pople as well as those, such as Penney, who came through theoretical chemistry to eminence in other fields. Several of the forty-five papers of his Cambridge period are written in collaboration with one or other of these men. They include a series of papers from 1935 to 1937 on the behaviour of atoms and molecules adsorbed on surfaces, then in 1937 a series on molecular orbitals applied to polyenes and aromatic molecules, then in 1938 with A. F. Devonshire a series on liquids. The series on molecular orbitals was the first to discuss the variations in bond lengths in conjugated and aromatic molecules from a theoretical point of view. The ideas which he used for this purpose have, not unnaturally, been considerably modified since; but the central idea of taking account of the sigma-bond orbitals in describing the pi-electrons is quite fundamental. His achievement both at this period and after the war lay not only in these published researches and in the help and inspiration that he gave to his students, but also in the masterly and clear expositions of modern ideas in theoretical chemistry which he always gave to research workers on the experimental side of the subject both in Cambridge and in frequent meetings elsewhere.

As a professor of a mathematical subject holding his chair in a department of chemistry rather than in the faculty of mathematics, Lennard-Jones held a unique position in Cambridge. Not only did he have a room in the laboratories—a privilege shared with the Plummer Professor of Mathematical Physics—but he was the head of a sub-department. This meant that in course of time he acquired a staff, small but highly talented, and rooms in which both his staff and his research students could work. The part that he took in the foundation of the mathematical laboratory in Cambridge helped him to achieve this. Before the war the work of his group on molecular orbitals had reached the point where it was clear that mechanical computation was becoming necessary. He therefore constructed a small differential analyzer, an account of which was published in 1939. Out of this grew the Cambridge Mathematical Laboratory, of which Lennard-Jones was director in the early years of the war. When after the war M. V. Wilkes took over this laboratory, Lennard-Jones was able to obtain rooms for his staff and students where they could sit and work, hold discussions and drink coffee, and where secretarial assistance was available—all to the immense advantage of the

group who were thus constantly in touch with each other, and who, like the members of a good laboratory, felt themselves to belong to a body with a loyalty of its own. One of his great services to Cambridge science was to build up within the framework of an ancient university the organization for theoretical research which was proving itself so successful elsewhere.

In 1946 on his return from war service Lennard-Jones threw himself into the task of building up again his school of theoretical chemistry. About fifteen papers belong to this period, on molecular orbitals and especially on the concept of equivalent orbitals. At this time he seldom had less than fifteen research students directly under him—a very considerable burden. But he was also very active in university policy. With the great expansion in the number of research students in Cambridge, many of them with only slight connexions with existing colleges, he strongly advocated the foundation of a graduate college. In this he was not successful. An innovation, however, which owed a lot to him is the provision of a University combination room, the only place in Cambridge outside the colleges where senior members of the university can meet and discuss their business together over a sandwich and a glass of beer. During the period of expansion after the war he was most concerned to form within the University a body competent to speak on matters of scientific policy, such as the desirable numbers of undergraduates and research students in scientific subjects and the organization of those branches of research which are less closely related to teaching. It was mainly on his initiative that an informal committee of science professors was formed, and Lennard-Jones acted as its spokesman in a long correspondence with the Council of the Senate on these matters. It was his view, in particular, that in any expansion of the University priority should be given to an increase in the number of postgraduate students and to the provision of proper facilities for them both in the laboratories and outside.

In the year 1946 he was knighted, and in the same year he was awarded the degree of Sc.D. by the University of Cambridge. In 1948 he was President of the Faraday Society and in 1953 he was awarded the Davy Medal of the Royal Society and the Hopkins Prize of the Cambridge Philosophical Society. From 1947 to 1954 he was a member of the Research Panel of the National Gallery.

Lennard-Jones's work during the war was as for so many others an interruption in his scientific career. At the beginning of the war the writer remembers that he remarked to him rather sadly how much less the theorist had to offer than the experimental man. But before the end of the war, with his wide knowledge of science, his administrative ability and his powers of leadership, he found that he had plenty to offer all the same. Soon after the outbreak of war, the University Mathematical Laboratory of which he was then director became closely allied to the External Ballistics Department of the Ordnance Board, and under his direction a staff was engaged there on problems of internal and external ballistics and other aspects of armament research. During this period he first became a member of the Scientific

Advisory Council (S.A.C.) of the Ministry of Supply and of several of its Committees, including the Gun Design Committee, the Unrotated Projectile Propellants Sub-Committee and the Sensitiveness of Explosives Committee. He also joined the Ballistics Committee and Internal Ballistics Sub-Committee and became Chairman of them both in June 1941.

In 1942 he was appointed Chief Superintendent of Armament Research, to take charge of the old Research Department (Woolwich) in its new role as the Armament Research Department, and his major contribution to the war effort was his work in this important and expanding research establishment as its technical and administrative head. With his wide background of scientific knowledge he was eminently suited to this task and although his experience of administration had been on the much smaller scale of a university he was rapidly able to get to grips with the details of this large government department. With a staff of over three thousand it is not surprising that he was unable to give any selected technical field particular attention and his work there must be judged by the results of the re-groupings which the changing demands of total war made necessary, a task made all the more difficult because his staff were distributed in many groups over the whole of the country.

He set up his headquarters at Fort Halstead, near Sevenoaks, and the changes he made in the administrative machinery have remained a permanent feature of that establishment, in particular in the large extension of its library and its facilities for publishing technical reports and information. He believed that scientists should be encouraged to put their names to the reports they wrote, somewhat against the former traditions of the department. He was able to secure the services of various scientists of distinction to take charge of some of the new groups he set up. One of these, an innovation in the establishment which has proved permanent, was a group for theoretical research in armaments, composed of men with a mathematical background, of which the writer became the first superintendent.

Another important change was the collection into one group under Dr C. Sykes of the various teams working on armour penetration; this was largely responsible for the issue to the Services of armour piercing projectiles much superior to anything possessed by the enemy. From January 1943 he was a member of the Explosives Research Committee of the S.A.C. and he also carried out a complete re-organization of the explosives research side of the Establishment. Later, when W. G. Penney selected the A.R.E. as the department in which to develop and build the first British atomic bomb, he found at his hand an organization easily adaptable to this task, and only as the work expanded was it found necessary to set up a separate establishment.

The staff of the Armament Research Establishment will remember Lennard-Jones as a chief who spared no time or effort in the advancement of the work and for the kindness and consideration which he invariably gave to all with whom he came into contact.

After the end of the war in Europe he remained in the establishment for

several months. At this time he was, as a member of the planning committee which made recommendations on the post-war organization of the Scientific Advisory Council, very much concerned with its future and with plans for post-war collaboration between the universities, industry and the establishment he was shortly to leave. During these few months also, he planned, in collaboration with Professor W. E. Garner, the new Explosives Research and Development Establishment at Waltham Abbey, designed to study the basic and chemical manufacturing problems associated with explosives and propellants.

He left the Armament Research Establishment in August 1945 to become Director-General of Scientific Research (Defence) at the headquarters of the Ministry of Supply, a post which he occupied until, in spite of the offer of several positions in government service, he decided to return to academic life at Cambridge in the autumn of 1946. During this time he was largely concerned with the re-organization of scientific research in the Ministry to adapt it to peace-time conditions, and with re-organization consequent on the merging of the Ministries of Supply and Aircraft Production. His interest in the collaboration between the universities and government establishments continued for many years after his return to Cambridge.

It remains to describe the last months of his life at the University College of North Staffordshire. Lennard-Jones had not intended to leave Cambridge; when he was offered the position of principal of the college he and Lady Lennard-Jones went up to see it, hardly expecting to be more than just interested. But the Keele experiment aroused his enthusiasm and after some months of hesitation he accepted the job. He felt that his experience as a scientist, as an educator and as an administrator could serve him well at Keele and that his deep feeling for the Christian religion would contribute to his task. He was sorry to leave his group of research students at Cambridge; though perhaps not sorry to be relieved of the burden of so large a number. He hoped that he would find time to write two books on theoretical chemistry, one a book addressed to experts in the subject and the other a more popular book meant for experimental workers in chemistry.

Of his work at Keele, so tragically cut short after less than a year, it is probably true that he had three things mainly in mind: the non-specialized teaching course which is the basis of the Keele experiment; the financial position of the college; and its relations with the outside world.

With his experience of Bristol and Cambridge behind him, one of the urgent tasks to which he applied himself was to devise in the new college an administrative system which would enable all members of the staff to play their appropriate part in the direction of the place, in particular by setting up a number of committees at all levels. At the same time he brought under review the curriculum of the college. A feature of the education at Keele is the course during the Foundation Year, which aims to give the first year student a broad view of human culture and modern civilization. It was clear to Lennard-Jones that a course like this provided the teaching staff with

great opportunities, but with dangers as well. At his suggestion a small sub-committee was set up responsible for improving the lecture content of the Foundation Year, and at his suggestion two weekly discussion groups, under the chairmanship of tutors of the college, were started for students attending the course. Moreover he was much concerned about the danger that students at Keele, owing to the width of the course, might suffer from an overloaded time-table of lectures and tutorials, and it was again due to his personal intervention that the number of hours devoted to formal instruction was reduced.

He had also to concern himself with the financial affairs of the college and to see that economies were affected wherever possible. He realized that in the early days of a new venture of this kind budgeting for the future was bound to involve much guess work, and that in the early days the essential thing had been to get the college working. But when he became principal towards the end of the third year of the life of the college, the time had arrived when he felt it desirable, on the basis of such experience as was available, to approach the college budget from a more realistic point of view. He devoted a great deal of his time to this particular problem, making it his special task to re-allocate funds wherever it seemed right to do so and to effect as much saving as possible.

He also felt that one of the problems facing a new college was to make itself known. He appreciated the interest felt throughout the country in this new experiment in general education and by written articles, by speeches and lectures and by broadcasting he did what was possible to bring the work and aims of the college to the public notice. During his period of office, too, the college organized a conference of directors of education, a development of a practice initiated the previous year of holding an annual conference of headmasters. In the spring of 1954 he went out of his way to make both headmasters and directors feel at home at Keele and their response and interest showed how his work was appreciated.

He was also very deeply concerned to build up a friendly relationship between the college and the people of the part of the country where the college stands, and both he and Lady Lennard-Jones were most active and hospitable in inviting guests to the Principal's House—staff, students, guests of the college, preachers and many representatives of the social, educational and business life of the area. During his short time as principal he did a great deal to promote good relations between the college and the people of North Staffordshire, who now have a much more sympathetic understanding of what universities in general and Keele in particular are trying to do.

In September 1954 he was President of Section B of the British Association for its meeting at Oxford, and received also from the University of Oxford the degree of Doctor of Science *Honoris Causa*. In October he was awarded the Longstaff Medal of the Chemical Society for 1954. He died at Stoke-on-Trent on 1 November 1954.

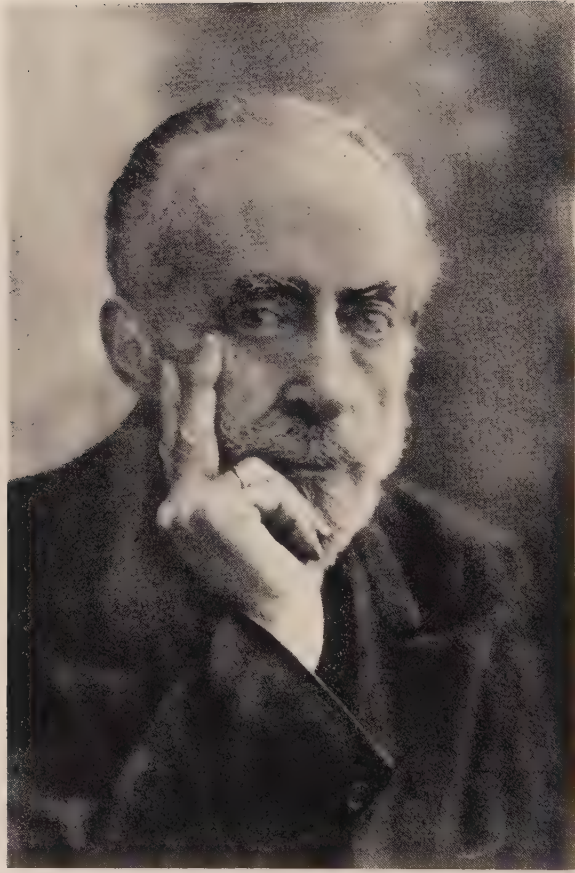
N. F. MOTT

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Louis de Marguerie

EMMANUEL MARIE PIERRE MARTIN JACQUIN DE MARGERIE

1862-1953

EMMANUEL MARIE PIERRE MARTIN JACQUIN DE MARGERIE was elected a Foreign Member of the Royal Society in 1931. He was born on 11 November 1862, and died on 20 December 1953 at the age of 91. As his christian names imply, he came of a family with a Roman Catholic tradition and both his parents had inherited modest wealth. His paternal grandfather, in youth, had shown courage in denouncing Napoleon's decision to bring to Paris a number of the masterpieces of Italian art after his victories in north Italy. His father was interested in humane learning and, at the same time, so open minded as to encourage his son's love of scenery and his drawings, bases of his future life work. An English nurse had had charge of his babyhood and had begun to teach him her language. At the next stage he and his sister Camille had an English governess, Miss Marian Farrell, of whom he spoke with deep affection. De Margerie used to speak of holidays among the mountains, of walks in Haute Savoie, of the famous silhouette of the white Pyrenean peaks as seen from the walls of Carcassonne under a blue sky, and of a teacher, M. Dupaigne, whom his father employed for him. M. Dupaigne was not only a remarkable teacher for a gifted boy, but also an alpinist who encouraged his pupil's study of scenery. De Margerie was not sent to the state schools, but teachers trained him privately, especially in languages, so that he came to use freely several, and most notably German and English, thus laying the foundation of an important aspect of his future fame, as the personification of internationalism in science. He lived in the family household until 1903 when he married the accomplished and devoted Mademoiselle Renée Ferrère, who predeceased him.

Before he was 15 he was already a member of the Geological Society of France and was attending (in 1877) the lectures of A. de Lapparent at what was then the Université Catholique de Paris, but had to change its name to Institut Catholique de Paris in consequence of a law of 1880, which restricted the name of university to government institutions. De Lapparent was a great and progressive teacher. His *Traité de Géologie* was long a standard work, and his *Geographic Physique* included an account of the geomorphology of the world, region by region; it still has considerable interest whatever changes more than 70 years of research may have made in interpretations. Naturally de Lapparent and his pupil became friends, and so began a long series of collaborative scientific friendships, which have been a great feature of de Margerie's life.

The first international geological congress was held in Paris in 1878 and it proved to be a decisive event in the development of the young geologist. He attended and met many workers in this field, but most significant of all was his finding the *Untersuchungen ueber den Mechanismus der Gebirgsbildung* (2 vols. and atlas, 1878) presented to that Congress by the famous pioneer, Albert Heim, professor at Zürich University. De Margerie decided that he would get to know Heim when he had grown up, and meanwhile he studied Heim's writings and probably those of the other pioneer Eduard Suess of Vienna whose book *Die Entstehung der Alpen* had appeared in 1875.

In 1880 a carriage accident made him rather lame for life. This rendered him unfit for military service, and it may have turned his attention to some extent to library work rather than field exploration, though, with the aid of his stick, he managed long walks and even climbs.

As he had not attended the state's schools or taken the Baccalauréat (University entrance examination) he could not proceed through the university to the Licence, Agrégation and Doctorat and was therefore barred from an official professorship, the traditional crown of a scholar's career in France. In spite of this, the republic later acknowledged his eminence by giving him an honorary professorship in the University of Strasbourg in 1919, along with his appointment to the directorship of geological services in the then newly recovered territories of Alsace and Lorraine. It is French custom that the Director of Geological Services is a professor seconded to this duty for a term of years, after which he returns to his professorial chair.

With modest wealth, de Margerie had been able to live quietly without permanent official commitments until 1919, and he had become a cultured man of science who read widely both in his own field of work and in general literature. It is of interest to English people that the appreciation of Indian thought by Max Müller and the works of John Ruskin had a special appeal for him. With the latter he shared a love of meticulously fine drawing.

Making his own field the geomorphological zone of overlap between geology and geography, he accumulated a library of relevant books and papers soon added to by authors from many lands who recognized in him the chief of all reviewers in the field he was cultivating. He dealt with all parts of the world, and formed the useful habit of writing review-articles giving the state of knowledge on the topic chosen, and usually referring to the most important live work on it. These articles were often long and very valuable clarifications of scientific problems and contributed notably to the international reputation so deservedly gained by the *Annales de Géographie*. He seems to have kept a copy of every review or article he wrote as well as a copy of every review of his own work written by others. All this was built up into an orderly collection accompanied by many letters from fellow-workers far and wide. Perhaps his long freedom from official commitments may have contributed to these methodical habits which have greatly helped the progress of science. His tidy mind was supplemented by a prodigious memory. Appeals for help in finding out what had been done on a particular subject brought a rich response and

sometimes induced confusion in the questioner who probably did not know many of the papers cited, often from memory, by de Margerie. He would give authors' names, names of journals, years of publication and a summary of contents and conclusions, also, not seldom, particulars of some review-articles of his own giving a critical appreciation of recent work on the subject in question.

To return to the subject of his friendships in work, he maintained his resolution to get to know Heim and achieved this about the time he came of age. They formed a solid collaborative association and Heim's letters talk about Alpine structure and refer to Suess whose work must have been known to de Margerie. Further, the first volume of *Das Antlitz der Erde* appeared in 1883; it was a real spring time of geomorphology, and the new development of thought was to gain a great deal from de Margerie's devotion.

Heim and de Margerie soon joined forces to produce *Les Dislocations de l'Écorce terrestre*, a work that aimed less at the advocacy of new theories than at the clarification of terms used in describing land forms and geological processes. Until then every writer had more or less chosen his own terminology and there was considerable confusion that the new work reduced. Heim was evidently deeply impressed by the knowledge and skill in bibliography shown by his young colleague. The work appeared in 1888; and, in 1948, at the International Geographical Congress in London it was decided to reissue this famous work, with revision where necessary.

De Margerie had already appeared in print in 1882 with a study of the geology of New South Wales translated from the English of C. S. Wilkinson at the request of de Lapparent.

Parallel with the partnership with Heim came another collaboration, with General de la Noë, head of the Geographical Staff of the French Army. Their work *Les Formes du Terrain* was deeply concerned with discussion of denudation, and de Margerie later helped to develop the study of longitudinal profiles of rivers.

Another friendship, again with a senior, grew between de Margerie and the thoughtful geographer and cartographer, Franz Schrader, beloved and revered in Britain as well as in France. The Pyrenees were a favourite field of study for Schrader and mention was made above of de Margerie's early impressions of their majesty. The partners seem to have walked and worked together among the mountains and by 1886 they were already writing papers on the subject. The '*Aperçu de la structure géologique des Pyrénées*' (1892) is the best known of these works. It agrees to some extent with conclusions of older geologists in tracing narrow structural belts over great distances lengthwise along the chain. But its main conclusions differ from those of all previous work in the region by showing that fan-folding has played a major part in the structural evolution and that horizontal dislocations, with overfolds, have been much more important than vertical dislocations through faulting. One may note parallels between this work and that of Charles Lapworth in the Scottish Highlands correcting opinions of Sir Archibald Geikie.

The *Antlitz der Erde* was meanwhile exercising its revolutionary influence on thought and de Margerie was drawn to think of a French edition. The original work in German had received some criticism because of the paucity of illustrations, however impressive the text was acknowledged to be. Part 1 had been issued by Suess in 1883, Part 2 in 1888 and the massive Part 3 in two sections came in 1902 and 1908 respectively. The French translation is scrupulously faithful to Suess's text, but, now and then, notes of more recent work are added, between square brackets. Bibliography is extended greatly and many new illustrations are added, while the lucid French prose is a great help to many a reader, though the German is not as complex as in some other learned works. De Margerie had many assistants in the great task, and *La Face de la Terre* added, as everyone wished it would, to the rightful fame of Suess. Even in the French edition illustration inevitably suffers from the weaknesses of the time at which it was issued. Had modern technique been available the results would probably have overwhelmed translator and publisher alike, even perhaps to the extent of putting off the completion of the great task for a generation. As it was Tome 1 appeared in 1897, Tome 2 in 1900 and Tome 3 in parts in the years 1902 to 1918. A critic who, de Margerie thought, questioned the integrity of Suess got a well-merited trouncing, in giving which de Margerie never forgot his own dignity.

In his study of the Ardennes, de Margerie was perhaps less surefooted than in some other fields. Older geologists had argued that the present outlines of the outcrops of Devonian strata there were largely the result of tectonic and physiographic changes of the time. De Margerie thought that Devonian rocks had once covered the whole area and that present distributions were results of subsequent denudation. It seems that, while recent opinion agrees in emphasizing the results of denudation, older views may not be altogether wrong.

Another aspect of de Margerie's work is connected with his friendship for Prince Albert of Monaco and the Institut Océanographique for which the Prince did so much. A bathygraphic atlas of ocean floors had appeared in 1903-4 partly under the aegis of this Institute. De Margerie and others had felt bound to criticize the maps somewhat drastically (1905). The Prince saw the need for revision and, in spite of some objections by persons who had been connected with the preparation of the first edition, he asked de Margerie to supervise the work, a task specially suited to a man of his meticulous care in draughtsmanship and bibliography.

Opposition and controversy have been mentioned above but they were only a small item in de Margerie's activities. He was however roused to wrath by attacks on the researches of Lauge Koch in Greenland. He followed polar studies and wrote a good deal on the subject though he did not travel beyond the Arctic or Antarctic circles except on one visit to Spitzbergen.

A long continued effort was that of his study of the Jura. Already at the age of 15 he had sketched a presentable map of the region and it occupied his attention at times during many years. One result was the 'Memoires pour

servir à l'explication de la carte géologique détaillée de la France—Le Jura' (1922 and 1936). Few regions in any country have received such detailed consideration.

De Margerie visited U.S.A. from time to time, to lecture or for consultations or to act as a visiting professor and W. M. Davis quoted G. K. Gilbert's opinion that the French geomorphologist knew more about the structure of North America than many a member of the Geological Survey of U.S.A. In his last published work 'Études américaines, Géologie et Géographie', he reissues a good deal from his earlier contributions. His comparisons between the Jura and the Appalachians are naturally of special intimacy. He was very much attached to his American colleagues and their country.

It is natural that, with his orderly mind and his vast knowledge of scientific journals and books in geomorphology and related subjects, de Margerie should be interested in bibliography generally and he contributed to the journal *Polybiblion*.

He had travelled in nearly every country in Europe save Portugal and, to his special regret, Greece. He knew a good deal of North Africa, but never crossed the equator and, strange to say, had not visited Asia, of the structure of which he knew so much. North America, north of Mexico and the Caribbean he knew specially well.

Mention has already been made of the great influence the first international geological congress (Paris, 1878) had on his mind and his career. For this reason it was natural that he should be specially faithful to these Congresses and he attended most, down to that at Algiers in 1952 when he was in his ninetieth year. He was honorary secretary at Washington (1891), Zürich (1894) and St Petersburg (1897), and often a Vice-President, but, strangely, he was never elected president. At Algiers, however, by acclamation, the veteran was designated *President d'Honneur*. At this Congress there was presented the completed Geological Map of Africa, the outcome of more than 30 years of work. De Margerie had been at first honorary secretary and later president of the commission entrusted with this task. He was the only member of the 1952 Algiers Congress who had been a member of the first, and one may add that he had been an active member of the Geological Society of France for nearly 77 years.

It is strangely difficult to put one's finger on any primary geological discovery or theory due to the mind of de Margerie, who nevertheless enriched so many fields of physical geographical and geomorphological research by his careful assessments and synthetic efforts. He kept workers of many nations and languages in touch with thought in other lands and was internationalist *par excellence*.

Clarification and completeness of survey were perhaps his main aims and his success was largely due to his single-minded devotion to his science and to his characteristically French power of lucid exposition.

The honours offered to de Margerie were so many that one can hardly hope to give a complete list. He was president of the Geological Society of France

in 1899 and again in 1919. In 1923 he became *Correspondant pour la section de Minéralogie de l'Académie des Sciences* and was thus linked with the *Institut de France* of which he became *Membre titulaire* in 1939. His lack of the traditional state doctorate, and consequently of an official professorship, had probably delayed his full entry into the Institut. It has already been stated that he became *Directeur du service géologique d'Alsace et de Lorraine* in 1919 and was then given the title of honorary professor in the university of Strasbourg.

The National Academy of Sciences, Washington, U.S.A., awarded him the Thompson Medal in 1923 and he was elected a foreign member of the American Philosophical Society in 1932. The American Geographical Society gave him the Cullum Medal in 1919.

Our British Geological Society gave him the Lyell Medal in 1921 and the Wollaston Medal in 1946, having made him a foreign member in 1897. Our Royal Geographical Society gave him its Victoria Medal in 1930. He greatly prized his Foreign Membership of the Royal Society, to which he was elected in 1931.

In the midst of the distress due to Nazi occupation of France, de Margerie, already about 80, received unexpected comfort and encouragement from an award made to him in 1943. Pope Pius XII had founded a prize for scientific work to be awarded by the *Pontificia Accademia delle Scienze* and, through the initiative of the Italian geologist, Giotto Dainelli, it was given to his veteran French friend and colleague. This was a welcome instance of the overriding of political opposition through scientific co-operation. Some other gifts supplemented this valuable prize and allowed de Margerie to go on with and to complete a great project he had begun and had feared he would be forced to give up. This project was nothing less than the re-issue in a collected edition of as much as possible of his scientific work and of reviews of that work by colleagues. The result is five large volumes with the general title *Critique et Géologie*.

Volume 1 (1943) gave his own sketch of his life work with a few personal references as well as tributes to teachers and senior friends, especially Heim and Suess. It also gave his papers and reviews on *Dislocations de l'Ecorce terrestre* and on *Formes du Terrain* as well as papers concerning *La Face de la Terre*. That vast effort of course was not itself included in the reprint.

Volume 2 (1946) gave papers and reviews relative to oceanography, to the cartographical aspects of which de Margerie's contribution in the drawing of contours had been outstanding. It also had his reviews and articles on polar regions and on cartography. Vidal de la Blache and he were two of the chief pioneer advocates for the international one-in-a-million map of the world.

Volume 3 (1946) gave his 'Études pyréennes', several in collaboration with Franz Schrader, notably the 'Aperçu' mentioned earlier in this article.

Volume 4 (1948) reissued a great deal of his work on the Jura and Volume 5 is the *Études Américaines* (published in two parts in 1952 and 1954) already mentioned. Interesting letters to de Margerie are often given in facsimile and there are many reproductions of photographs of scientific friends. There is

singularly little controversy in this unique memorial left by de Margerie's own efforts in a form in which it can be most conveniently used for reference.

M. de Margerie's *Critique et Géologie* in its five volumes thus not only gives a bibliography of his work but, as has been said above, reproduces a great deal of it.

H. J. FLEURE



E. M. Ellanby

EDWARD MELLANBY

1884-1955

DEATH came suddenly, and without recognized warning, to SIR EDWARD MELLANBY on 30 January 1955, before he had completed his 71st year. Nearly six years earlier he had retired, on reaching the official age-limit, from the Secretaryship of the Medical Research Council, which he had held for 16 years. During this period the range and the influence of the Council's activities for the promotion and support of medical research had undergone a most notable expansion, primarily in the United Kingdom, but widely beyond it also in the British Commonwealth; and Mellanby's enlightened and resourceful enterprise in proposal and planning, and his vigorous drive in administrative action, had undoubtedly been dominant factors in this remarkable development. Like his only predecessor in the appointment, the late Walter Morley Fletcher, Mellanby had been the Council's chief executive officer in much more than a merely official sense. Yet, during all the years in which he had carried that heavy load of official responsibility, he had succeeded in maintaining a direct and personal activity in the researches which had so long provided for him, and also for his devoted wife and scientific partner, the central aim and interest of their joint working lives. He could not, of course, have done this without the Council's active encouragement. When they invited him to become their Secretary, after Fletcher's untimely death in 1933, they had recognized, indeed, their duty to reduce to a minimum the inevitable interference of the duties of that appointment, if he accepted it, with the progressive contribution to medical knowledge which the Mellanbys had been making in Sheffield since 1920, working there in a research unit maintained largely by a grant from the Council's own funds, with additional support from the University and from the local medical community. That local attachment could clearly not be long maintained if Mellanby's primary duties were thenceforward to be in London; so the Council undertook to provide, on an estate, much of it then still vacant, which they owned at Mill Hill, a new laboratory especially designed for the purpose, to which, as soon as it could be built and equipped, the unit from Sheffield could be transferred. Mellanby might then again have ready access to his own research facilities, in such marginal time as might be left to him from his new duties. And, after some years of inevitable delay for planning and construction, during which the researches, even after Mellanby's removal to London, were still kept in being at Sheffield and actively pursued and stimulated during week-end visits there, the new

laboratory at Mill Hill had been completed and occupied. And, there, during the remaining years of his Secretaryship, even during the war years with all their new and tremendous preoccupations, Mellanby had found relief from the demands and responsibilities of organizing for the researches of others, and of giving advice and stimulating action to meet a wide variety of abnormal and urgent problems, by returning at every week-end to the peaceful air of his own laboratory, to work again at his own scientific problems with his own hands and brain. The conditions which made this possible were clearly exceptional. His own specially trained assistants were there during the week to carry forward much that he could leave to them, and their working times could be adjusted to meet his own requirements; and, even much more important, of course, Lady Mellanby was working regularly in the laboratory, and supervising the conduct of what was being continued for him, while carrying on her own special work. With all allowance, however, for those favourable factors, it remained a matter for general astonishment, that Mellanby could not only continue to do effective and productive research with such intermittent opportunity, but that he appeared even to find, in the week-ends spent so busily, a release and a refreshment, a recreation of his powers, comparable almost to that which others would find in complete rest, or in physical exercise in the open air.

When Mellanby retired from the Secretaryship, the Medical Research Council were glad to be able to arrange for him to continue his researches in the special laboratory for a further period, with full time at his disposal. He was thus at Mill Hill on the morning of Sunday, 30 January, to observe and record the results of experiments, in which he was then in happy and fruitful collaboration with Dr Honor Fell, F.R.S., of the Strangeways Laboratory, Cambridge.* Satisfied with the morning's yield, he had gone out to a small garden adjoining the laboratory, where he was cultivating fruit and flowers; and it was there that death suddenly overtook him, relaxed from an immediate and exhilarating contact with his researches.

Parentage and education

Edward Mellanby was born on 8 April 1884, in West Hartlepool, where his father, John Mellanby, was the manager of the shipyard of the Furness-Withy Company. Edward was the youngest of a family of six children, being the last of four sons, of whom one died in childhood. All the other three showed scientific ability of such an order that, at one time, they were simultaneous occupants of important academic chairs. The eldest, Alexander Lawson Mellanby (d. 1951), was Professor of Civil and Mechanical Engineering in the Royal Technical College, Glasgow. The second, John, who became F.R.S., had occupied for some years a London University Chair in the Sherrington School of Physiology at St Thomas's Hospital, moving then to succeed Sir Charles Sherrington in the Chair of Physiology at Oxford,

* Later information indicates that Mellanby on his last morning was working on a new line of experiments, which he intended to report to Dr Fell at their next meeting (see also p. 210).

but dying (1939) before he had held it long, and still at the height of his powers. Edward, as we shall see, held in succession a London University Chair of Physiology at King's (now Queen Elizabeth's) College for Women, and the Chair of Pharmacology at Sheffield University,* before he accepted his last and greatest opportunity of service to medical science, with the Medical Research Council.

The parents were members of a free-church communion, and the children were brought up in an atmosphere of evangelical piety, with austere standards, no doubt, of conduct and enjoyment, but tempered and enlivened by a robust independence and a genuine regard for intellectual achievement and enterprise. The father, John Mellanby, came from Yorkshire and his wife, Mary Lawson, Edward's mother, was born in Edinburgh. John Mellanby senior had been an amateur boxing champion of the North of England, and, naturally, taught his sons to box. It is on record that he was once asked how all his boys had done so well at school and university, and that he replied that only one can be first in a competitive examination, and that he knew no reason why a son of his, if a candidate, should not be that one. The life of the Mellanby household must certainly have been of a character favourable to the creation and strengthening of close family ties; for his friends in after years could not fail to become aware of Edward's strong feeling of affection for his brothers and sisters, his nephews and nieces; of his pride in their achievements, and his generous concern for their troubles. This staunch and active family loyalty was, indeed, a conspicuous and attractive feature of his character.

After early schooling in West Hartlepool, Edward Mellanby, at the age of 14, followed his brothers to Barnard Castle School in 1898, with the aid of a bursary, which each of the others had held before him; and he remained there till July, 1902. At that time the headmaster was the Rev. Francis Lloyd Brereton, and Mr Edward Dalrymple Walrod was the senior science master. Mellanby retained a grateful memory of what he owed to these teachers, and until Mr Brereton died he kept in friendly touch with him. His school record gives the evidence which we should expect, of ability and natural leadership. He won the Upper School Prize and a special Prize for Theoretical and Practical Physics, and he became Head Boy of the school. I think, however, that those who knew him only in later years would have been a little surprised, to find that he left at the school at least as great a reputation as an athlete. He won several events as a jumper and a runner, and was captain of both cricket and football; he was described in the School Magazine as 'the best outside left we have had for some years', and became 'Victor ludorum' in his final year. Professor J. H. Burn, who entered the same school in 1903, tells me that it was Mellanby's fame as an athlete which then survived there, strongly enough to attract the notice of a new boy. These early athletic triumphs on a relatively small stage would not, of course, have justified predictions of conspicuous achievement on a wider one. On the other hand, for one who had given such early promise in games and must have enjoyed them

* This post was combined with that of Honorary Physician to the Sheffield Royal Infirmary (see p.204).

keenly, Mellanby was rather exceptional in the completeness with which, very soon after going to Cambridge, he ceased to take an active part in any, though he always kept an interest in the successes of others, and in watching first-class events on occasion. In the later years his time and his thoughts must have become so preoccupied in other directions, that he could hardly have found leisure for games, if the inclination to play them had lasted. The school-time athletic activities, however, had doubtless contributed to the development of his handsome presence and well-knit figure, and of the robust physique and equable health which made it possible for him, during most of his active career, to work with so tireless a zest, with so few remissions, and without interruption by any serious illness, except for one brief period towards the end of the war in which signs of overstrain compelled him to take a rest.

In October, 1902, Mellanby entered Emmanuel College, Cambridge, with an Open Exhibition in Natural Science and a Leaving Exhibition from the school. His brother John had been at Emmanuel before him, so that the way was eased for his reception there and for the making of contacts, some of which were to mean much to him. Of particular importance was his association, thus early, with Frederick Gowland Hopkins, then acting as Medical Tutor to the College, as well as University Lecturer on Chemical Physiology. Hopkins, as teacher and friend, was to have a decisive influence on the direction of Mellanby's career. It was, in fact, the anomalous failure of the Adamkiewicz colour-reaction for proteins, with the particular bottle of acetic acid allotted to John Mellanby in Hopkins's advanced practical class, a few years earlier, which had led Hopkins more or less directly to the discovery of tryptophane. And this was to lead him in due course, while Edward Mellanby was still in close touch with him at Cambridge, to investigate the number and the nature of such pure constituents, needed to make an artificial diet adequate for maintenance and growth, and thus eventually to come upon the trail of the 'accessory factors' or 'vitamins'. And Mellanby himself would have been the first to acknowledge, proudly to claim indeed, that his close contact with Hopkins during this period, when the new idea of the detailed, qualitative adequacy of a diet was being born or engendered, was a most potent determinant factor in his own later research plans and ambitions.

Mellanby's student career showed a record of the straightforward successes which were to be expected, with college prizes to mark them. He was placed in the first class of both parts of the Natural Sciences Tripos, his special subject for the second part being physiology. Perhaps the attraction of Hopkins's teaching and inspiration would have led him to choose biochemistry, if the subject had by that date achieved recognition in Cambridge as a separate discipline. We cannot be certain, however, that he would have chosen thus; for, though all the researches of his later career had a background of biochemical ideas, his approach to his problems was essentially from the side of biology and experimental medicine. In a preface, partially drafted at the time of his death, to the Chemical Society's *Annual review of biochemistry*, and

containing a glowing tribute to the influence of Hopkins, he was still urging with great conviction the importance of maintaining an effective co-operation between biochemistry, on the one hand, and experimental biology and clinical medicine on the other.

I myself first met Mellanby while he was a Cambridge student, and before he graduated. It was in the summer of 1904, and I was visiting his brother John, who was then finishing an appointment which he had held for a few years in the Wellcome Laboratories, while waiting for a studentship to enable him to return to Cambridge. And there, in John Mellanby's laboratory, I first met Edward, and was immediately attracted, as many others must have been, by the vigour and the straightforward friendliness of his manner, and the evidence of an active and independent mind. I had been prepared for this favourable first impression by what Hopkins had told me of this 'second Mellanby' who had come to Emmanuel; and it was an impression which all later encounters were to deepen, though these were not to be really frequent till we become officially associated, nearly 30 years after this first meeting.

Rumours had reached me also of some of his doings and interests at Cambridge, outside the range of his formal studies. The University Natural Science Club—a small group of the more serious students and recent graduates in that faculty, meeting in one or another's rooms on Saturday evenings to hear papers and indulge in discussion—appeared to have been rather shocked, though evidently not disagreeably, by a paper which the young Mellanby had read to them on the subject of 'Sin'—presented, no doubt, from what in those days was an unexpectedly wide, biological angle. There was even a rumour—probably with no real foundation—that the fame of this discourse, spreading beyond its proper audience to certain College authorities, had had an adverse effect on Mellanby's prospect of election to a Fellowship. In any case, the talk about it gave strength to my own impression of a new recruit to science, with a courageously enterprising mind.

Researches

With the tripos behind him, and a Research Studentship at Emmanuel to support him, the young Mellanby entered at once, in 1906, upon the activities in research which, with only such interruption as that due to the further clinical studies required for medical qualification, were then to continue for nearly 50 years till his life ended in 1955. In that later period of his scientific career which we should, perhaps, regard as its culmination, this direct and personal activity in medical research was to be overlapped, as mentioned already, by his simultaneous achievements as the chief official promoter and organizer, in this country, of opportunities for a great army of other workers in that general field. But, while it should further be recognized that his services to medical research along each of these two lines could not have been without an effect of some kind upon the other, it will be convenient here to follow their courses separately, and to deal first with the personal researches, beginning with the completion of his formal studies at Cambridge.

Research at Cambridge

In the review article mentioned above, which he left unfinished, Mellanby recalled the conditions under which he served his first apprenticeship to research, in 1906-7. In the old Physiological Laboratories at Cambridge, only one small room, with incredibly poor equipment by modern standards, could then be assigned for any researches in that department which dealt with the chemical aspects of the subject. Yet it was in this room that some of Hopkins's most brilliant researches had been done, and were even still in progress; and it was in it, alongside Hopkins, that Mellanby also was to work for the next year. The inevitable inconveniences of such overcrowding, however, must clearly have been substantially offset by the privilege of working in such regular and stimulating contact with Hopkins and his ideas; for Mellanby's output, in this first year of his research experience, showed no sign of being hampered by the conditions, even though they were such that no research worker to-day would regard them as tolerable. It is significant, I think, that Mellanby's first effort in research, while he was still thus working in Cambridge under the direct influence and guidance of Hopkins, dealt with an application of biochemical methods to a definitely medical problem, in investigating the changes produced by diseases in the excreted products of a certain line of metabolism. A published abstract of his first communication to the Physiological Society in 1907, the year after he finished his Tripos examinations and when he was 23, dealt with 'The excretion of creatin and creatinin in hepatic disease'. Neither cirrhosis nor venous engorgement of the liver, he found, caused any important departure from the normal condition, in which creatinine only of these two bases is excreted in the urine; in cancer of the liver, on the other hand, much of this normal creatinine is replaced by creatine.

And a year later, in 1908, came Mellanby's first substantial publication, on 'Creatin and creatinin', in which the whole question of the origins and reciprocal relations of these two bases in the vertebrate animal body was re-opened, and thoroughly investigated. A number of ideas then current, and supported in some cases by eminent authority, were re-examined by newly available and better methods, and decisively rejected. The conclusion was drawn that creatinine is the direct product of the primary metabolism taking place in the liver, while creatine is normally produced from creatinine for storage in the voluntary muscles; and, in accordance with that conception it was found that in very young animals, whether newly hatched chickens or newly born babies, there is no excess of creatinine left to be excreted, until this capacity of the muscles for storing it as creatine has been satisfied. Mellanby's work did not touch, of course, the question of the function of this creatine in muscle, the nature of which did not come to light till many years later; but there was abundant promise of further achievements in this paper, the record of a young man's first full trial of his hand at research, with results making possible the clear and decisive removal of the grounds for a series

of misunderstandings and controversies, which, till then, had encumbered and obscured its subject.

Before this first full-length paper was published, Mellanby, in 1907, had left Cambridge for London, to complete his medical course by clinical and pathological studies at St Thomas's Hospital. Even before that, he appears to have gone to Glasgow during a Cambridge Long Vacation, to take a course in pathology under Professor (now Sir Robert) Muir.

Research at St Thomas's Hospital

During the next three years we find no record of further publications dealing with research. Then, in 1911, he published a paper on cyclical vomiting, describing clinical observations made on the condition and using, in connexion with it, his earlier experience on the metabolism of creatine and the significance of its appearance in the urine. He considered also the possibility that the vomiting might be connected with some cyclical change in the intestinal flora, leading to an excessive production of histamine, the powerful activities of which, and its occurrence in the intestinal mucosa, had then recently been made known by work in my own laboratory. Having by then graduated in medicine and obtained a position as demonstrator in the Department of Physiology at St Thomas's Hospital, and having become thus interested in the possible effects of bacterial activities in the intestine, Mellanby began a collaboration with the late F. W. Twort, of the London Hospital, already a man of great experience in bacteriology, and one who was later to make discoveries of great importance in that field. Twort was then acting as Superintendent of the Brown Institution, in South London. Together they isolated in pure cultures, from intestinal contents, more than one strain of bacteria which attacked such substances and produced such changes as to give them a special interest for Mellanby. A strictly anaerobic, Gram-staining bacillus was thus found, which rapidly and completely destroyed creatine; and the extent to which this action might occur in the intestine clearly required to be taken into account, in assessing the results of metabolic experiments in which creatine was given by the mouth. A little later, in this joint enterprise with Twort, a bacillus of the typhoid-coli group was isolated, growing either aerobically or anaerobically on ordinary laboratory media and actively decarboxylating histidine to produce histamine. Mellanby and Twort's main deduction from this was that the histamine which Barger and I had found, as a constituent of the washed mucosa of the intestine, was probably the product of such bacterial action. The suggestion was plausible enough at that time, though it was later made much less so by the discovery of histamine in many other tissues. Mellanby continued for some years to be interested in the possible participation of histamine, if produced by conditions favouring the growth in the intestine of the appropriate bacteria, in the genesis of such conditions as infantile diarrhoea and vomiting; and he published a paper presenting evidence in favour of such a possibility as late as 1916. Mention should also be made of another investigation in

which he used biochemical methods and clinical opportunity for a study of the metabolism of lactating women, the results of which were communicated to the Royal Society, and published in its *Proceedings*.

The date of the publication last mentioned, 1913, brings us to another and major turning-point in Mellanby's scientific career. So far, it may be suggested, he had been largely feeling his way into the life of research, guided by a general instinct to use the biochemical methods, in which the association with Hopkins had awakened his enthusiastic interest, for the study of problems having some bearing upon the conditions of human health and disease. During this trial period he had held a demonstratorship and, later, a Beit Memorial Fellowship. The resulting researches, without leading to particular discoveries likely to attract wide notice, had clearly given abundant evidence of his capacity for dealing with a variety of problems, with devotion and accuracy, with alertness of interest, and with a resourceful ingenuity. He was marked already, in short, as a recruit of great potentialities to the then relatively small army of the devotees of medical research, waiting for a problem worthy of his powers and his equipment, and for an opportunity to give him adequate scope.

At King's (now Queen Elizabeth's) College for Women

The opportunity was to come in a quarter which might have appeared to be even curiously detached from the clinical association in which Mellanby had so far tended to find his subjects for research. He accepted appointment, in 1913, to a Chair of Physiology of the University of London in the King's (now Queen Elizabeth's) College for Women, founded to provide an education in household science with a genuinely scientific basis and a full academic status. The course for its students of physiology might, accordingly, be expected to be unusually detached, in its aim, from the requirements of clinical medicine, though its bearing on the general health would be clear enough. The research opportunities, though otherwise good, could obviously not be expected to facilitate clinical contacts or co-operation. Mellanby, however, like the rest of us in those days, had to take the opportunity for research where he found it; and his desire to establish himself in a career, without further delay, would naturally be quickened by the prospect of marriage. For a student friendship with May Tweedy, at Cambridge, had early ripened into a clear understanding that they would marry, as soon as conditions made their union wise and reasonable. Miss Tweedy herself, meanwhile, had made a promising start in physiological research at Bedford College, London; and so they were married, in the year following Mellanby's appointment at the King's College for Women, and began their long and happy partnership.

Research on rickets

Mellanby had not long been settled in the new position and opportunity, when a subject of research presented itself, worthy of all his effort, and

congenial to his instinct for a biochemical approach to a problem of human disease. The Medical Research Committee (predecessor of the Medical Research Council), then newly appointed to administer the first of such Government Research Funds, held a meeting in the early part of 1914, at which I was present by invitation, to discuss possible schemes for expenditure. The proposal of the nature and causation of rickets, as a suitable subject, was put forward by Hopkins as a member of the Committee, and was at once accepted; and the meeting proceeded immediately to discuss apparently numerous possibilities, each of which might be investigated by a suitably qualified worker. The field seemed widely open; but Hopkins made a special case for asking somebody to make an experimental study of the metabolic defect in rickets, and recommended that Mellanby should be invited to undertake this; and both these proposals were very readily accepted. I remember, too, that with his shy hesitation—and it may well have been only in a whispered aside to me—Hopkins even speculated on the possibility that rickets might prove to be due to a lack of one of the new ‘accessory food factors’, or ‘vitamins’. This, however, was clearly not meant for transmission to Mellanby, who told me, indeed, in after years, that no such suggestion reached him; and the First Report of the Medical Research Committee, for 1914-15, records only (p. 26) that ‘Dr Mellanby was given part-time work, with an assistant supplied by the Committee, for the study of experimental rickets and its relations to conditions of oxidation’. Whatever that may have meant, it clearly offered no direct hint of a dietary defect of any kind as the cause; and Mellanby had yet to effect a great clearance before he was able, about 1916, to find and to follow that clue for himself.

Mellanby was soon at work, therefore, on the problem of rickets, so challenging and, at the same time, so congenial to his interests. The extensive histological and biochemical investigations were largely carried out in the King’s College laboratories, at Kensington; but accommodation had to be found for many of the dietary trials on dogs, which provided the experimental back-bone of the research, in the Field Laboratories of Cambridge University. A large series of controls was needed to deal with the competing claims which were still being made for lack of exercise, for forms of infection, etc., as causes of rickets, and then to obtain the clear evidence, which Mellanby was eventually able to produce, first for a dietary deficiency as the primary and essential cause, and then for something present in animal fats, specially abundant in cod-liver oil, but lacking or weakly represented in vegetable oils, as the factor which must be present in a diet. Rickets regularly appeared when this factor was absent or deficient, and was rapidly cured by its administration. It corresponded closely in its distribution with what was then still known as ‘the fat-soluble vitamin A’, containing, of course, as was discovered some years later, the carotene-derivative now known as vitamin A, as well as the product of irradiation of certain sterols, now recognized as the essentially antirachitic vitamin D. Infections, lack of exercise and all the other suggested causes of rickets, were thereby firmly and finally relegated to

their proper role as contributory factors, capable of weakening the protective effect of marginal quantities of the antirachitic constituent in the diet, but not of causing rickets when that protective factor was given in a normal abundance. And it may be noted, in passing, that there was no suspicion yet of a production of the antirachitic factor by ultra-violet rays; so that there could be no consideration at that stage of the possibility that liberation for exercise, if it allowed any considerable exposure of the animals or their food to sunlight, might accidentally supplement a deficient antirachitic component of the diet.

Altogether then, this work of Mellanby's, the first really scientific attack on the problem, with its long series of carefully planned and controlled experiments, had effected a great and most wholesome clearance of a mass of conflicting arguments and observations, which had hitherto obscured and confused the issue; and, in so doing, it had placed the essential factor properly in the centre of the picture. When once the real nature of the cause of rickets had thus been discovered and presented, this main position was never again to be seriously threatened, in spite of certain obstinate rearguard actions in defence of earlier beliefs. On the other hand, the way was now open, especially with the added facility afforded by the experimental dietary production of rickets in rats, for others to separate the vitamin D, to link its production with the antirachitic effect of the ultra-violet rays, and, many years later, to isolate and identify more than one such vitamin D from the products obtained by irradiating different sterols. It was Mellanby's laborious and accurate experiments on dogs, however, which had enabled him to make the initial and crucial discovery. And this had not only become the essential starting point for all the later developments; it had already provided knowledge which, by itself, and without waiting for these further and consequential advances, could be directly and easily applied to the prevention and cure of human rickets, till then so disastrously prevalent among the children of the urban populations of our industrial civilization.

Mellanby's first public mention of what he had discovered about rickets was made in short notes, recording communications made to the Physiological Society, at the beginning and the end of 1918; and his first full report on his findings, to the Medical Research Council, did not appear till 1921. The time of these announcements, at and soon after the ending of the First World War, could not be regarded as propitious for a quick recognition of their importance. Nevertheless, the success with which the discovery was applied in practice was not only remarkably complete, but even dramatically rapid. Rickets, as a serious menace to the general health, was so soon and so effectively eliminated, that the direct memory of its former malign prevalence, and of its prompt disappearance, is already beginning to fade. Not many years later, when it became necessary to assay the potency of the first pure vitamin D (Calciferol) directly, in the treatment of human rickets, a search made in all the London hospitals failed to discover a single case in which the mother, guided by a welfare clinic, had not already given effective

treatment with cod-liver oil. The search had to be carried to a pocket of unemployment in the industrial North, before a few suitable cases could be found. I have always regarded this work of Mellanby's as one of the high peaks, one of the outstanding points of new departure, in the revolutionary advance of therapeutic knowledge and practice by research during the present century—comparable in its direct success, and in its stimulating influence on further enterprise in research, to the earlier discovery of salvarsan, and the later ones of insulin and penicillin. Of course, when once the discovery had been made scientifically, it was recalled, by Mellanby among others, that cod-liver oil had earlier been suggested as useful in rickets, more than one surgeon being credited with having recommended it for lion cubs at the Zoo. It was beyond dispute, however, that such empirical indications had had no serious effect on theory or practice in human medicine, whereas Mellanby's evidence was such as to carry immediate conviction, leading to prompt and effective application.

Further researches

As was to be expected, the immediately important results of this, Mellanby's first major undertaking in research, made it the starting point for investigations, too numerous even for an estimate, by workers all over the world and along a number of different lines. And he was content to leave to a succession of such other workers the further separation and, eventually, the complete chemical identification of the anti-rachitic vitamin. Of his own further research activities, those which we may regard as the four principal ones—on a pro-rachitic dietary factor, on the pathology of nerve degenerations caused by a deficiency of the vitamin A and consequent anomalies of bone development, on a toxic action of flour conditioned with 'agene', and on the effects of an excess of vitamin A on embryonic organs in artificial culture, all these will call for separate mention, in relation to the further successive phases of his career. And it may be noted that these major lines of his further researches can all be traced in origin, more or less directly, to questions already opened by his first work on rickets. Reference, however, to the full bibliography at the end of this notice, will show that this was by no means true of all the different experimental enquiries on which, at different stages, he embarked. It will not be possible here to make even separate mention of all these, additional and unrelated as they were to the main nutritional current of his researches. Those interested, for example, in Mellanby's experimental approach to the cancer problem, must be referred to the relevant original papers, which are fully listed.

Absorption and fate of alcohol

One of these aberrant research enterprises, however, and the earliest of them, should receive a more specific, though brief, mention. The late Viscount D'Abernon, as Chairman of the war-time Liquor Control Board, was anxious to have all the well-founded scientific evidence about the action

of alcohol made easily available. The Scientific Committee, which he had appointed, found that more definite information was needed about the comparative rates of absorption of alcohol into the blood from different kinds of drinks, and when taken in different relations to food of various kinds. I think that I was responsible for suggesting that Mellanby should be asked to undertake the required experimental study, with the support of the Medical Research Council. In any case, he readily accepted the invitation. The research, carried out at the end of the war, partly at King's College for Women and partly at the London Hospital Medical School, was very successful in its prompt production of clear-cut and definite information, on the different points which he submitted to experiment. Lord D'Abernon himself found great pleasure and interest in the visits which he paid to the laboratories in Whitechapel, to see the Mellanbys at this work and to discuss the possible significance of its results for his Board's policy; so that when Mellanby became Secretary of the Medical Research Council some fourteen years later, Lord D'Abernon having then become Chairman of that body, they started their official relationship on a footing of friendly confidence, already long established.

Professor of Pharmacology at Sheffield

It will, of course, not be possible here to make individual mention of the researches by other workers than himself, which grew more or less directly from Mellanby's work on rickets. Even Lady Mellanby's highly important researches, pursued with such enthusiasm and success for so many years, on analogous nutritional mal-developments of the teeth and the relation of these to liability to caries, must be regarded as an independent enterprise from the point of view of this Memoir, although it was carried out, of course, in close consultation with her husband. Here we must restrict our detailed consideration to the further researches on which Mellanby himself was already actively engaged, and for which his removal to Sheffield in 1920, to occupy the Chair of Pharmacology, was soon to give greatly improved facilities. Apart from the normal laboratories of his University Department, he was now to have the field laboratories which have been already mentioned, at a convenient distance; and it had further been arranged with the University and Hospital authorities that, as Professor of Pharmacology, he should be given the honorary rank of Physician in the Sheffield Royal Infirmary, with beds at his disposal, enabling him to extend his investigations and his teaching into the clinical field. The offer of this clinical opportunity was, indeed, an important factor in Mellanby's decision to accept the Sheffield invitation out of several which he received at that time. This interest of Mellanby's, in clinical opportunities for research, was not, in fact, to find extensive representation in his original publications. Perhaps the nearest approach which he made to a clinical discovery, arose from an observation made in the early days of his nutritional studies on dogs in Sheffield. He noticed that many of the dogs on the poorer kinds of diet, and

especially on those which did not include cod-liver oil, developed goitres, the histology of which was like that seen in Graves's disease. He found that cod-liver oil had, indeed, a remedial effect on the goitres in such dogs, and tried it then with success on Graves's disease in human patients, with the addition, following an indication from the work of Marine, of small doses of iodine; and then, in co-operation with Dr Cowell, a more systematic study was made of the favourable, though impermanent, effects of small doses of iodine alone, and a description of the results was published in the *Quarterly Journal of Medicine*, in 1929. So that Mellanby was one of those to recognize and investigate, at an early stage, the iodine treatment which was to prove so valuable, especially as a preparation for surgery, in exophthalmic goitre.

Mellanby was also responsible for one of the first clinical applications of insulin in this country, after its discovery by Banting and Best. Dudley and I, after a visit to Toronto, had circulated details of suggested methods for preparing and standardizing emergency supplies of insulin on a laboratory scale, while its factory production was being organized. Mellanby was one of the first to act on this suggestion, and was able, with insulin prepared in his own laboratory, to treat a sufferer from diabetes in an advanced stage, who, being rescued thus from a death which could not otherwise have been long delayed, is now a successful and prominent citizen of Sheffield, active and generous in his support of medical enterprises.

The pro-rachitic action of cereals

The first problem which was tackled by Mellanby under the new conditions, provided at Sheffield, and which was destined to occupy much of his attention for years, arose from an observation which he had made and recorded in his first research on rickets. He had found that rickets became more easy to produce, more severe, and more difficult to prevent or to cure, with increase of the proportion of bread, flour, or cereals of any kind in the diet. There was no question here of the removal of a vitamin by milling, for white flour was less rachitogenic than whole-meal flour, and oatmeal was the most pernicious of all. His own earlier work had already made it clear that rickets was not due simply to the absence of the protective factor, but to some dietary imbalance which that factor was needed to correct, and which might require more or less of such correction, according to the composition of the diet in other respects. Now, with the vitamin D identified as the protective agent, he came to the conclusion that there was a positively toxic constituent of cereals, and of their germs in particular, which directly antagonized the effect of this vitamin, requiring it to be added in corresponding excess to prevent or cure rickets. He had referred to this antagonist as a 'toxamin', and for many years he was engaged in trying to track it down, having found early that it was easily destroyed by acid hydrolysis. An understanding of the many aspects of this research must be sought in the original documents, and especially in Mellanby's second 'Report on experimental rickets' to the Medical Research Council, and his later papers on the subject, one with

Professor D. C. Harrison, in 1939, and the last by himself alone, in 1946.

A first clear indication of what proved, eventually, to be the real clue to the nature of this pro-rachitic effect of cereals, was given by Bruce and Callow in 1934, in describing a relatively limited study which they had made—as a sequel to their contribution to the isolation of Calciferol—of the conditions affecting the production of rickets in rats by a high calcium-low phosphorus diet, and the action of the vitamin D upon it. They found, essentially, that when this unbalanced ratio between the total calcium and total phosphorus in the diet was kept constant, the inclusion of cereals made the imbalance effectively worse, and thereby increased the rachitogenic action, because the phosphorus which these contributed to the total was in the very poorly absorbable form of salts of phytic acid—inositol-hexaphosphoric acid. The significance of this observation, made on the rickets thus produced in rats, for the interpretation of Mellanby's imposing array of evidence, obtained from dogs rendered rachitic by diets with a low calcium-high phosphorus ratio, was not immediately obvious. Mellanby with his co-workers, however, proceeded to make further studies directed to the testing of its relevance; and thus he came to discover that phytic acid and its salts, as contained in cereals, was also, indeed, the cause of the pro-rachitic effects of adding these to the low-calcium diet of his dogs. The presence of phytic acid, he found, restricted also the absorption of calcium by forming the unabsorbable calcium phytate. McCance and Widdowson had also extended the significance of this effect to human diets, showing that the absorption of calcium by man could be definitely impaired by the phytates in brown bread. In the end, therefore, it was clearly established that the phytic acid in cereals was responsible for the 'toxamin' effect which Mellanby had discovered, so far as this effect was pro-rachitic and antagonistic to the vitamin D.

Effects of deficiency of vitamin A; nerve degeneration and bony malformation

Many years before the nature of this pro-rachitic effect of cereals had been finally elucidated, Mellanby had embarked upon the third of his major programmes of research. The separation of the vitamins A and D had raised, for him, the question whether all the symptoms which he had observed in the dogs with rickets were due to the lack of vitamin D, or some of them, perhaps, to the inevitably associated lack of vitamin A in the experimental diet. As soon as vitamin D became available in a state of purity, and vitamin A as its precursor, β -carotene, he could begin to put the possibility to the test of experiment; and it thus became clear that a deficiency of vitamin A alone could produce a severe ataxia, which could be traced to degenerative nerve changes, affecting chiefly the sensory fibres, and most conspicuously those of the cranial nerves of the special senses, the olfactory, optic and auditory (8th) nerves, and some of the upper spinal nerves, together with central tracts corresponding to these. Later, these degenerative changes were traced to distorted developments of bones of the skull and the vertebrae, with resulting compression of the nerves, and of their sensory ganglia in particular, at the

foramina by which they pass from the cranial cavity and spinal canal. And these bony mal-developments were traced again, in remarkably beautiful detail, to displaced and unbalanced activities of the osteoblasts and osteoclasts. Here again, reference must be made for full details to the original publications; and, in this instance, an admirable first-hand account of the researches and their results is readily accessible to Fellows of the Royal Society, through the publication in *Proceedings B* (1944, **132**, 28) of the brilliant Croonian Lecture which Mellanby gave in 1943. A large proportion of the researches therein presented had been the product of his work at week-ends, during war years, in which his week-days were crowded with administrative demands and national responsibilities. It was my privilege on that occasion to comment, from the Chair, on that astonishing achievement, and on the special conditions which had made it possible, even for him. This study of the results of vitamin A deficiency, while less immediately dramatic, in its consequences for the general health, than the earlier work on rickets, must surely take very high rank among all Mellanby's major contributions to medical science.

Canine hysteria and 'agenized' flour

It was in 1946, when Mellanby was still giving full-time service to the Medical Research Council as their Secretary, so that his opportunities for research continued to be practically restricted to his week-ends at the Mill Hill Nutrition Laboratory, that he found the cause of a troublesome condition, long known to dog-fanciers and veterinary surgeons as canine 'hysteria', or 'running fits'. There had been no indication, so far, as to whether this condition was due to an infection, or to some other and unknown cause. Mellanby was able first to trace a regular association between outbreaks of it among his experimental dogs, and diets containing high proportions of wheat flour, as bread or dog-biscuit. Enquiry then produced evidence that the flours so incriminated had all been 'improved' and bleached by the so-called 'agene' process, consisting essentially of its exposure to a dilute vapour of nitrogen trichloride. Since a very high proportion of the flour used in this country was treated by this process, the publication of Mellanby's finding excited a natural and anxious interest, in circles widely beyond those normally concerned with scientific research. In any case, the discovery was of a kind to give it more of the 'news value' of a brilliant piece of detective work, than most of those which fall to the lot of the research worker. It was obviously of pressing interest to know whether this toxic action of 'agenized' flour was limited to the only species, the dog, in which its effects had so far been observed. Mellanby found that, of the animals readily available for experiment, only the ferret showed a sensitiveness to the poisonous product similar to that seen in the dog. There was no evidence at all of any comparably acute action of the agenized flour on man, at any rate in the quantities in which it would be taken in a human diet; but there remained, and still remains, an open possibility that some chronic deleterious action may yet be

found to be associated with its continued use. Mellanby's striking experimental evidence had certainly given a signal of warning to all those responsible for public health policies in such matters; and at the end of 1954 he was writing to Lord Hankey to congratulate him on the reward of his 'missionary zeal', in securing the abolition of the 'agene' process.

He also took the first steps towards the identification of the poisonous constituent, showing that it was produced by action on the gluten of flour, that it was resistant to acid hydrolysis when it had once appeared, but was no longer formed from the products of a preliminary hydrolysis, if these were exposed to the 'agene' treatment. After that stage he wisely shared the further chemical investigation with others who had the special experience and facilities which it required. With Campbell and Work, of the National Institute for Medical Research, the poison was separated and characterized as a product of some methionine-complex in the gluten; and workers for the British Millers' Research Association, under Dr Moran, then further identified it as methionine-sulphoximine, and confirmed the identification by synthesis. Mellanby thus had the satisfaction of seeing an investigation of peculiar interest, which had had its origin in his own alert use of opportunities for observation and his brilliant initiative, brought early to a definite conclusion.

Experiments on embryonic organs in artificial culture

In the first two years after he retired from his Secretaryship, 1949-50, Mellanby's expected freedom for research in the Nutrition Laboratory at Mill Hill was rather compromised by his acceptance of invitations, first to India, and then to Australia and New Zealand, on advisory missions lasting for three months, or more, in each case. He was able in those years, nevertheless, not only to finish his own active part in the agene research, but even to devote increasing attention to a new venture, in which he had begun to use the method of studying the growth of isolated embryonic organs in artificial media. His immediate object was to examine further the mechanism of the action of vitamin A in promoting an orderly development of the bones in the living young animal, and of the abnormal development resulting from its deficiency, which he had discovered in the researches earlier mentioned. Did the vitamin act directly on the tissues of the growing bones, or on some other organ, responsible for a nutritional or endocrine action required for the normal development? Some preliminary experiments were made to observe the effects, on embryonic bone rudiments, of growing them in artificial media deficient in vitamin A; but these, for some reason, gave negative results. Mellanby then arranged to continue the work in co-operation with Dr Honor Fell of Cambridge, who had an unrivalled experience of the artificial cultivation of isolated organs and tissues, and of its details and possibilities. They decided, in the first instance, to make the easier experiments, in which the bones of embryo chickens and mice were cultivated in media containing an excess of the vitamin A. The result was dramatic. The

matrix of the cartilage of the rudiment lost its characteristic staining properties and then disappeared, and the shaft of the bone shrank and disintegrated, till nothing was left of the supporting matrix but crumbs of debris, scattered in a sheet of actively growing and migrating cells, derived from the former cartilage and bone cells. This process was not a necrosis; it did not occur if the bone rudiment was dead; it appeared rather to represent an over-stimulation, by the excess of vitamin A, of one aspect of a normally balanced physiological complex, in which the earlier cartilaginous matrix is absorbed as new bone is laid down round it. The effects thus observed were, in fact, in line with already known effects of an excess of dietary vitamin A, in producing an abnormally rapid absorption of cartilage and bony tissue in the bones of young animals. The effects now seen *in vitro* made it clear, therefore, that this action was due to a direct effect of the vitamin on the components of the skeleton, and not an indirect effect of its action on some other organs.

In the next phase of this study, dealing with direct effects of the vitamin A on other growing tissues, reactions were encountered which had still more of the exciting character of the unexpected. It was known that deficiency of vitamin A in a living animal produces, among other effects, a squamous metaplasia and keratinization of mucous membranes. It was accordingly of interest to observe the influence of the vitamin on the development in isolated culture of ectoderm from a chicken embryo, taken from such a part of the body that it would normally acquire the character of a stratified epidermis, with a surface layer of horny scales. In explants of such ectoderm grown in a normal medium, this development took place in a typical manner; and deficiency of vitamin A in the medium did not prevent this and might even accentuate it. In media containing excess of the vitamin, on the other hand, similar explants had failed after a week's cultivation to show signs of the keratinizing change, and the cultivation was continued further, to see whether this inhibition was only temporary. After about ten days a layer of sticky secretion had formed on the surface of the explants, and chemical tests showed that this had the character of mucin. When the explants were then examined histologically, the unexpected discovery was made that the ectoderm had been transformed, not, as normally, into a stratified epidermis, but into a columnar epithelium, containing goblet cells which produced the mucous secretion. Still more surprising, even exciting, was the discovery that other cells of this epithelium were bordered with actively beating cilia, similar to those of the epithelium of the chicken embryo's respiratory tract. Also unexpected, perhaps, was the further observation that, when such explants were then transferred, from the medium containing an excess of vitamin A, to a normal medium, the mucus-secreting and ciliated cells of the columnar epithelium were not replaced from below; on the contrary, they ultimately died and were shed, the basal cells having meanwhile formed a squamous, keratinizing epidermis underneath them.

These studies of the direct effects of vitamin A on epithelia were extended to other structures of ectodermal origin, the otocysts and retinae of 4-day

chicken embryos; but no comparable metaplastic changes were observed in these, though the effect on the cartilaginous capsule of the otocysts was like that on other cartilages.

An offer of further collaboration from Dr S. R. Pelc, of the Medical Research Council's Radiotherapeutic Research Unit at Hammersmith, enabled the analysis of the direct effects of vitamin A to be carried a stage further. By the use of sulphates containing radio-active sulphur (^{35}S), and thin autoradiographs from sections of the tissues, so superimposed on the actual sections that both could be seen together under the microscope, it could be demonstrated that the uptake of sulphur by cultivated explants of ectoderm and bone rudiments, and its distribution in their tissues, are profoundly modified by the addition of vitamin A to the medium.

Mellanby had earlier begun a study of the effects of thyroxine also on isolated bone rudiments in culture, and had observed an inhibition of growth, especially in the long bones of the leg. When the co-operation with Dr Fell began, they found that, as *in vivo*, the addition of thyroxine at first accelerated the development and maturation of the cartilage in the explanted bone rudiments. Later it induced degenerative changes; but these appeared to be due to an excessive stimulation of the metabolic requirements of the growing tissue, since they were checked by increasing the glucose in the medium. The observations were extended to the action of tri-iodothyronine, which was found to be similar to that of thyroxine, but about three times as potent, as in comparisons of their activities *in vivo*.

The mention of these last collaborative investigations brings us to the point at which Mellanby's activities were so suddenly cut short by his death. The experiments on the effects of thyroxine and tri-iodothyronine, and those on the effects of the vitamin A on sulphur metabolism, were still in progress when he died, though nearing completion in both cases. We are left with a conviction that this new field, into which, by a natural and logical development from his earlier work, he had extended his researches in these last years, was, and still is, rich with promise of further harvests of discovery.

Even at the time of his death Mellanby had some new experiments in hand, in which he was using developing chicken embryos and tissue cultures for further observations on the actions of methionine-sulphoximine, the poisonous product of the 'agene' treatment of flour.

Last thoughts on and plans for research

The essential novelty and the special interest of the results, which his own most recent work had thus continued to produce, gave no indication that Mellanby was not still at the height of his powers as an investigator. He seemed to have lost none of his gift for instinctive recognition of the lines of advance likely to be practicable and rewarding, or of his eagerness to exploit the new methods, which the general advance of scientific knowledge was so rapidly making available. It might have been hoped that he would still have some working years before him, in which to take a principal part, with both

hand and brain, in the further development of this new scientific territory into which he had led the way, and to enjoy, perhaps, a growing co-operation with younger colleagues, able to contribute specialist knowledge of the new techniques which would certainly continue to emerge. In his last report to the Medical Research Council, on the researches in which he had been engaged since his retirement from the Secretaryship, he had appeared to envisage, as a desirable policy, an increasing use of such opportunities of co-operation, with workers attached to other research establishments receiving the Council's support. It is sad that an enterprise so full of promise should have lost his stimulating initiative and the guidance of his ripened experience at so early a stage of its progress; but we may be glad to think of him as enjoying to the full, and to the last minute, this brilliant 'Indian summer', at the end of so many years at productive research.

Reference has already been made to the special circumstances which rendered it possible for Mellanby to continue his researches without a break, through the period in which other pre-occupations could hardly have failed otherwise to make an interruption. Throughout their married life, indeed, though each of them during most of it had his and her own separate problems and plans for research, Lady Mellanby and he had worked alongside one another with such an intimacy of understanding, that the effects upon his research work of prior calls on his time and attention could be reduced to a minimum. It should further be made clear, in the same connexion, that an account of his research career would be seriously defective, without a tribute to the devoted and understanding co-operation which he received from a succession of highly qualified technical assistants; and special reference should be made to the skilful and versatile contribution thus made to his work, especially in the later years, by Mr R. J. C. Stewart.

Secretary of the Medical Research Council, 1933-1949

When Sir Walter Fletcher died, in 1933, the Medical Research Council's undertakings, starting on a very small scale less than twenty years earlier, had already undergone a remarkable development during his enterprising and enlightened Secretaryship. There was no reason to suppose that this expansion of the Council's activities had yet been completed; it might, on the contrary, be expected to continue at an increasing rate, with similarly able guidance. It was clear, then, that the appointment of a successor to Fletcher would be one of the greatest importance to the prospects of medical research and its support from the public funds in the United Kingdom, and beyond it in the British Empire. When it was learned, therefore, that Edward Mellanby had accepted the appointment, there was a widespread feeling of satisfaction, that a man of his well-known ability and force of character, and with so direct and intimate a knowledge of the needs and the possibilities of medical research, had consented to shoulder this heavy burden of responsibility for the promotion of medical science, and for the interests of his fellow

research workers. For some, however, this feeling of relief and approval was tempered by one of regret, for what appeared to them to be the practically inevitable sacrifice of Mellanby's own very important personal activities in research—even though it was known that the Council had undertaken to enable him to continue his researches with their full support, concurrently with the discharge of the responsible duties of their Secretary. I confess to having been among the sceptics; however sincere the intentions, it seemed to me that, with two such different claims on his thoughts and interests, one or the other must suffer from the attempt to meet both at once, and that the claim of his new appointment would inevitably win. How wrong I was in this judgment has already been made abundantly clear. I do not think, however, that it was wrong in principle, or in a general application; my mistake was to have underestimated so widely what Mellanby, in particular, would be able to achieve.

Mellanby's task as Secretary did not, of course, involve a start from nothing, as Fletcher's had done. He had succeeded to a well-developed organization, with an able and enthusiastic staff already experienced in its working. He was not the man, however, to be content with the mere consolidation of such an inheritance, and he soon began to give proof of his capacity for a vigorous and independent initiative. It was evident that, in response to his stimulus, the range of the Council's enterprises would expand in new directions, and that there would be pruning where it was judged to be needed, as well as planting. A survey in detail of the researches promoted, or reinforced, by support from the Council's funds, even during the first six years of Mellanby's Secretaryship, before the war, would involve the listing of a very large proportion of all the medical researches then in progress in this country, as recorded in the Council's Annual Reports. There were some who appeared to believe that they could detect certain new general tendencies in the Council's policy, and that these, not unnaturally, were of a nature congenial to Mellanby's own interests and ideals in medical research. They would watch, for example, for indications of a growing tendency to encourage research in the clinics, and on problems, such as those of nutrition, which might have a relatively immediate bearing on the conditions of human health. I believe, however, that, if there was really any such change of emphasis in the Council's policy, it could with equally good reason have been attributed to a natural alinement with a more general, indeed a world-wide tendency of medical research at that time. What was certainly clear was that, while the Council, with Mellanby's prompting, might well be giving additional attention to investigations more directly related to practical medicine and health policy, they were not doing so at the expense of their support for more fundamental studies. Mellanby's influence, in fact, was clearly in favour of casting the net widely, taking the long view, and making the Council's encouragement and support available to any well conceived and expertly manned enterprise in the whole field of medical research, whether it promised to yield an early harvest of practical results, or only to enrich the soil with

fundamental additions to knowledge. I may briefly mention examples, out of a great range from which choice could be made, of research projects of these different kinds, which attracted the Council's support during Mellanby's term of office, and which I know that he viewed with special pride and satisfaction. The Council were directly concerned, on the one hand, with the first organized clinical trial, under conditions of scientific control, of the then new 'Prontosil' and its active moiety, sulphanilamide, in the special unit which they had founded for research on puerperal fever, under the direction of Dr Leonard Colebrook; and the result was so decisive as to awaken medical interest all over the world in the potentialities of sulphanilamide, and then in those of its more active derivatives. Some three years later, the Council were again interested, from an early stage, in promoting the new investigation, by Sir Howard Florey, Dr Chain and their team, of the properties of Fleming's then almost forgotten penicillin, resulting in its separation and purification, and in the triumphant demonstration of its supreme remedial value in so many general infections. The Council's active concern with these, the two chief starting points of the revolutionary advance, still continuing, in the treatment of bacterial and similar infections, was largely due to Mellanby's prompt and enthusiastic recognition of their immediate and prospective importance. I know that Mellanby, on the other hand, would have found equal reason for pride in the foundation by the Council, during his term as their Secretary, of the unit of research under Professor Krebs, originally in Sheffield, and recently transferred with its leader to Oxford, for the fundamental study of the enzymatic mechanisms of oxidative metabolism. The 'Krebs cycle' is now well known, as a major component of the rising structure of fundamental biochemistry, and has been recognized by the award of a Nobel Prize and a Royal Medal of the Royal Society; but it required an enlightened vision to foresee the eventual importance of researches of that order for practical medicine, even in a distant future. And we who were members then of the staff of the National Institute for Medical Research, had abundant reason to know that the Council showed no abatement of its interest in the more fundamental aspects of medical research while Mellanby was their Secretary.

Mellanby, like Fletcher before him, was engaged, during an important part of his secretarial term, in the special organization of medical research to meet the needs of a world war. In both cases the Council and its staff were called upon to meet a forced extension of the demands for their activities, and therewith of their opportunities and resources; and, though many of these war-time activities were of special kinds, they had yielded, being medical in their general aim, an unusually high proportion of new scientific knowledge and experience which would have a more permanent, peace-time value. The Council had accordingly emerged, from each of the wars, with its range and resources greatly enlarged and its reputation greatly enhanced, so that there was no demand for a shrinkage of its activities to the former dimensions; and, in both cases, it was evident that a major share in a widely

applauded achievement was attributable to the resourceful and devoted labours of the Council's Secretary.

In other ways the experiences of the two had differed; unlike Fletcher, on whom the 1914 emergency had been sprung without warning, and only a month after he had taken office, Mellanby had six years of service in which to watch the advancing shadow and to prepare for the call, before it came in 1939. He had been busy at an early stage of his new appointment, making contacts with the official authorities in the different relevant departments. A very important contact of that kind was with Sir Maurice (now Lord) Hankey, who was then Secretary to the Committee of Imperial Defence, and to the Cabinet, as well as Clerk to the Privy Council, to which the Medical Research Council is officially responsible. In a moving address, which he delivered at the Memorial Service which followed Mellanby's death, Lord Hankey has told of an early appeal which he made to Mellanby, for some action to improve the standard of health among men presenting themselves for military recruitment; and of Mellanby's reply that 'the problem could be solved, but only on a long-term basis, and by drastic reform in the national diet'. Lord Hankey asked for a memorandum; and, to Mellanby's astonishment, a day or two later it came back to him in the form of a Memorandum to the Cabinet. This heartening experience, coming to Mellanby at a moment when he was feeling baffled 'by the well-nigh insuperable difficulty in getting research work translated into action', opened the way to an invaluable collaboration behind the scenes, on confidential preparations for the demands for medical research, and for its products, which would be made immediately, if and when the threatened war began. A more open preparatory move, of importance not only for war, was the appointment by the Air Ministry, in response to Mellanby's vigorous representations, of a committee to deal with the physiological problems of aviation. From what I heard from the other side, the officials of the Ministry thought that they had 'trumped Mellanby's ace', by insisting on his becoming the chairman of this committee; but I have no doubt that he regarded the trick as taken by himself. When the war came, therefore, much of the planning, for the organization of special medical researches from the Council's office, had already been done. Professor Topley, from the London School of Hygiene and Tropical Medicine, joined the headquarters staff, and an Emergency Public Health Service was organized, in collaboration with the Ministry of Health, as Sir Arthur McNalty has recalled.

Mellanby himself was constantly at work behind the scenes and on advisory committees, pressing successfully, and with scientific warrant, for the appropriate reinforcement of margarine, bread and other staple articles of diet. The long delay of active belligerency, after war had been declared, gave time to put the prepared machinery into action, and special research committees on medical problems of the war came into being, in such number and variety as to preclude any attempt at detailed reference. Special mention may be made, however, of the new emphasis on physiological problems

concerned with the maintenance of health, apart from measures against infection and other known causes of disease, in men who would be exposed to a wide variety of abnormal stresses and conditions, on land, or sea, or in the air; so that a range of expert committees was appointed, to consider and advise upon normal and emergency rations, clothing for a wide variety of climates, maintenance of fitness in marching, or during service in submarines or tanks, problems of deep diving, and on many others newly created by the changing conditions of warfare.

The war had helped to give Mellanby a position of power, and opportunities of service on high-level official committees, which did not disappear when it ended. One of his most important interdepartmental functions, until he retired from his position as the Council's Secretary, was that of Chairman of a Committee on Colonial Medical Research, appointed jointly by the Medical Research Council and the Colonial Office. As he gained the confidence of the departments, he was more and more consulted on their various medical problems.

Among other matters of the Council's policy during Mellanby's Secretaryship, mention should also be made of the decision to remove the activities of the National Institute for Medical Research to a new building, to be erected on their estate at Mill Hill. The eventual need to increase and improve the Institute's accommodation had long been recognized; and action was accelerated by the announcement of a special increase of the Government's annual grant, to make possible a large extension of the programme of researches on chemotherapy, in response to a memorandum which Mellanby had presented to the Treasury on behalf of the Council. Important new accommodation for chemotherapy would evidently be required by the Institute, and it was administratively undesirable to provide it at a distance from the main building. So, after a number of abortive attempts to plan appropriately for further building on the Hampstead site, the decision was taken to move the Institute as a whole to new quarters at Mill Hill. Building eventually began not long before the outbreak of war; but when the main carcass was completed, in 1941, and before work on the internal fitting and equipment could be begun, it was requisitioned for what was regarded as a prior war-time need—the preliminary instruction of successive flocks of recruits to the W.R.N.S. And so it came about that the new Institute, with its internal planning and equipment redesigned, to its great advantage, by my successor Sir Charles Harington, who was able to take advantage of many great advances due to war-time researches, was not ready till 1950 for its formal opening by the late King George VI and his Queen; and there was general regret that this should not have been possible till a year after Mellanby's retirement from the Secretaryship. For it was during his term of office that the decision for this new building had been made, and its planning and construction, after the war-time diversion, had been brought very nearly to completion before he retired.

Other activities and distinctions

Much could be written about other aspects of Mellanby's work. Apart from the researches which he undertook from time to time, in addition to those of which a more detailed mention has been made, some reference is due to his qualities as a teacher. The obligations of formal teaching and organization, in connexion with the university chairs which he held, were probably lighter than those of many, and thus left him with more freedom for his own researches. Nevertheless, he obviously enjoyed the contact which regular teaching gave him with young and eager minds, and letters from his former students have given glowing testimony to the attractive, unconventional and inspiring quality of his lectures; while those who had the privilege of collaborating in his researches have spoken, with a like enthusiasm, of his unassuming friendliness and open-hearted sharing of experience and ideas, and of what they learned from him of the principles and the spirit of scientific research. And reference may be made to the attached bibliography for evidence of the extent to which he was in demand as a special lecturer, both at home and abroad.

His greatest titles to fame, however, will still be found in the record of his own major enterprises in research, and of his service to medical research in general, as a great administrator and public official. Other instances could, no doubt, be cited, beginning perhaps with that of Isaac Newton, of men who, having risen to great eminence as scientific investigators and discoverers, have later found opportunity to show a different aspect of their powers, as great public servants. It was Mellanby's special title to greatness that, having achieved high rank as an investigator of great originality and distinction, he continued to hold it, by maintaining the high level of his own activity in research, when he became, in addition, a great administrator of the public funds provided for the general support of research in his own field of the medical sciences, and a most determined and forceful advocate, in official circles and widely beyond them, of the proper use and application of the results of such research, for the promotion of health in the nation and throughout the world.

Nobody, I think, who had dealings with Edward Mellanby could fail to be attracted by his big, handsome, friendly personality, which, in his happier moods, had retained something of a boyish exuberance. His character was strong and independent, and, when he considered the opinions of others, he looked at them on their merits and with little reference to the standing or the dignity of their advocates. In most cases he appeared to form his own opinion; and when he had done so, and taken a stand, you were likely to find it difficult to move him from it if you happened not to agree with him; but you could be certain that it was his own and not one tamely accepted from others. I think that, like other great men with a sense of a mission, he was apt on occasion to weaken his own influence by faults of manner, which were essentially due to an excess of good qualities. When his convictions were

strongly moved, or if he felt that his plans were being hampered by a conventional timidity, his reaction could be explosive and provocative, when it might have served his purpose better to be persuasive and adroit. Perhaps the early discipline in boxing with his father had left its mark on his reactions; for, if one of his outbursts was met by a counter-attack of like pugnacity, he took it in good part, and he cherished no grudge, if, at the end of an encounter, he had to accept defeat. He was staunchly loyal to his own co-workers; but his well-justified pride in the effectiveness of the organization which he had been called to administer, and in what it was achieving as it continued to grow under his hand, was apt to overflow into criticism and resentment of efforts by other and less experienced organizations to play a part in the promotion of medical research. But nobody who knew Mellanby at all could attribute such a reaction to anything but an excess of his consuming zeal for the great cause which he had espoused, or could suspect him of any concern for his personal interests. He was a doughty champion of the interests of medical research, as he saw them, and a man of great personal achievement in its performance; and, with it all, he was a man of simple, generous and lovable character, who could be tender in his sympathy for a friend in trouble, as well as fierce and impetuous in his drive for what he believed to be right and important.

Mention has been made of advisory missions to India and Australia which Mellanby undertook after his retirement. Even earlier, in 1947, leave from his official duties had enabled him to accept an Abraham Flexner Lecture-ship at Vanderbilt University, Nashville, Tennessee, for which the lecturer is under obligation to reside for three months in Nashville, so as to have opportunity for informal contacts and discussion with the staff of the University, in addition to the delivery of more formal lectures. He was still in office also in 1948, when he attended, on behalf of the British Government, the meeting in Washington of a Commission appointed to consider the practical implications of his own discovery concerning the effect of the 'agene' treatment of flour. As the result of the Commission's report, the 'agene' process was prohibited in the U.S.A. The British Government accepted the recommendation in principle, but, as already mentioned, some years were required to make it effective. In 1948 again, he went to South Africa to advise upon the organized promotion of medical research at the invitation of the South African Council of Scientific and Industrial Research, and in 1949, still before his retirement, he attended the African Scientific Regional Congress on behalf of the Colonial Office.

Among many important Committees of which Mellanby was Chairman, special mention should be made of a conference under his chairmanship which was convened in London by the Health Section of the League of Nations in 1931, when he was still a Professor in Sheffield, to consider the standardization of the vitamins already then known; with the result that a number of international standards for these substances were accepted, and units of biological activity defined in terms of them. Mellanby continued to

preside over subsequent conferences, summoned in 1934 and 1949, to keep these important international agreements abreast of further advances in that field. He was also Chairman of an International Technical Commission on Nutrition, and, in co-operation with Professor E. V. McCollum, of Baltimore, he produced a very influential report on the relationship of human nutrition to agriculture.

In addition to the Abraham Flexner Lectures already mentioned, Mellanby gave the Croonian Lecture to the Royal Society, the Oliver Sharpey Lectures, the Croonian Lectures, and the Harveian Oration to the Royal College of Physicians, the Linacre Lecture and the Rede Lecture at Cambridge, and the Withering Lectures at Birmingham. The Royal Society awarded him the Royal and the Buchanan Medals, and he served on its Council in 1932-1934 and 1953-1955; from the Royal College of Physicians he received the Bisset-Hawkins, Moxon and Baly Medals. He was Hon. Sc.D. of Cambridge; Hon. D.Sc. of Sheffield, Oxford, Belfast, Chicago and Oslo; Hon. LL.D. of Birmingham, Glasgow, St Andrew's and Melbourne; Hon. M.D. of Witwatersrand and Adelaide. He was a Fellow of the Royal Danish Academy of Sciences.

Mellanby was created K.C.B. in 1937, and G.B.E. in 1948. In 1947 he became Officier de la Légion d'Honneur (France); he was also awarded the American Medal of Freedom with Silver Palm, and made a Commander (1st Class) of the Swedish Royal Order of the North Star.

The portrait facing p. 193 is from a photograph taken by Elliott and Fry Ltd in 1927, when Edward Mellanby was 43 years of age.

I am deeply indebted to Lady Mellanby and, through her, to many others, for help with information and reminiscences, which have greatly contributed to this notice. The attached bibliography was most kindly prepared for me by Miss Jeannette R. Taylor, Librarian to the National Institute for Medical Research.

HENRY H. DALE

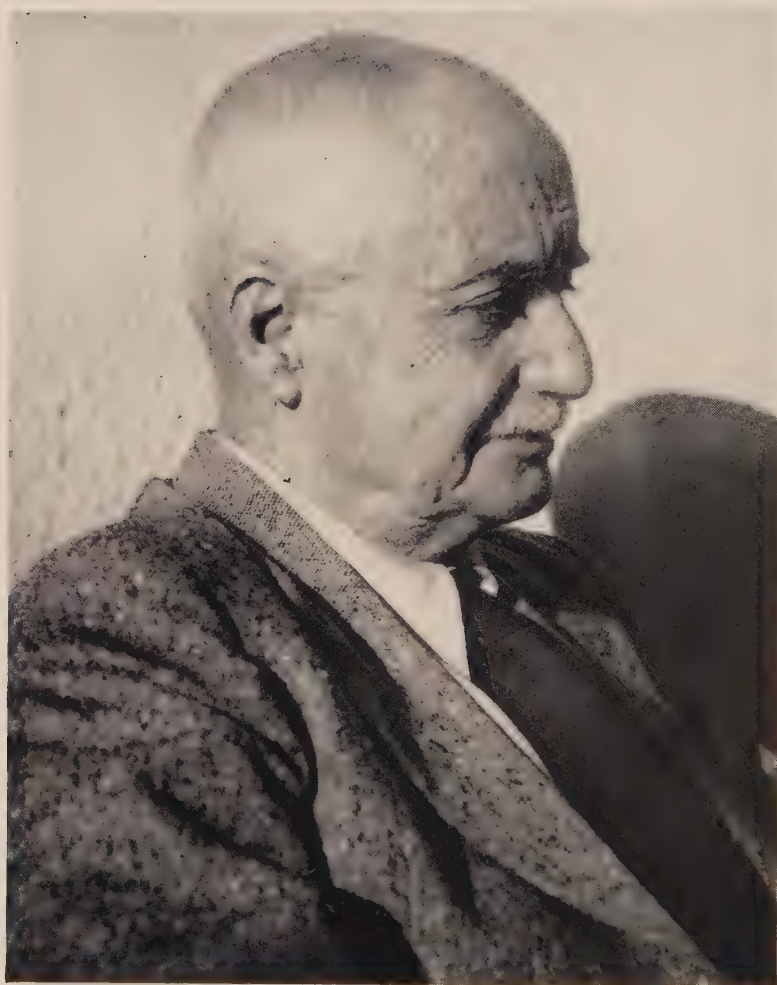
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ARCHIBALD READ RICHARDSON

1881-1954

ARCHIBALD READ RICHARDSON was born on 21 August 1881 of English parents: he was the eldest of five children and was dearly loved by the whole family. The greater part of his life before he went to Swansea in 1920 was spent in London where he began his academic career in 1903 as an engineering student at the Royal College of Science. As a volunteer (probably under age) he had already served in the South African war, on one occasion during which members of his troop, although unable to swim, had to get their horses over the flooded Tugela river at midnight. His college course, 1903-1907, was one of sustained distinction, and he developed an interest in pure mathematics which attracted the attention of his teachers, particularly of Professor L. N. G. Filon, who wished to appoint him forthwith to his staff as an assistant in mathematics. But Richardson, it would appear, was not yet a member of the University of London; and to become a lecturer required his passing the next matriculation examination: this done he was duly appointed. Three years later he obtained First Class Honours in the external examination. From 1912 to 1914 he was assistant professor in mathematics at the Imperial College, which had been formed by the amalgamation of the Royal College of Science, the Royal School of Mines, and the City and Guilds Engineering College. In August 1914 he left for war service and returned in April 1919. In August 1920 he became Professor of Aeronautical Science at the Cadet College, Cranwell; and shortly afterwards, in October 1920, he was appointed to the Chair of Mathematics in the University College of Swansea, the newly created constituent college of the University of Wales. This post he held for twenty years until he was compelled to resign in 1940 on account of increasing ill health, when he retired to Cape Town, but continued to prosecute research until shortly before his death in 1954. He was elected a Fellow of the Royal Society in 1946.

Richardson to an eminent degree combined the qualities of a man of action with those of a scholar. His character was strong, vigorous and clear-cut; it matched his constitutional robustness and vitality. He was a rugby footballer (he told a friend he had twice broken an arm at this game), a cricketer, a climber, and an active member of the Territorial Army, in the University of London Officers' Training Corps. He was appointed to a commission as 2nd Lieutenant in 1909, and at the outbreak of war in 1914, went on active service with the British Expeditionary Force to France, rising to the

rank of Lieutenant Colonel. Early in 1915 both he and Professor Filon were seconded to the 2nd London Regiment—the Royal Fusiliers—and Richardson took command of the 2nd Battalion Londons before they went overseas in 1916 with the 58th Division. In charge of one and a half battalions of the 2/2 and one battalion of the 2/4 Londons, he relieved the 59th Australians and went on to capture and hold part of the Hindenburg line in what became known as the Battle of Bullecourt. For this action Col. Richardson received the D.S.O., June 1917. He was posted as missing after the bitter fighting of 21 March 1918 near Farguier. Conan Doyle (*History of the London Regiment* and *The British campaign in France and Flanders*) describes it: ‘The single battalion (2/2 Londons) in front was attacked by the impossible odds of three German divisions, but held out for a long time with great constancy. Their brave Col. Richardson was last seen surrounded by the enemy, but still fighting with his headquarters troops about him.’ During the action he received a very severe bullet wound which left him an invalid for life. A friend writes: ‘He was an outstandingly brave and masterly soldier, displaying the greatest forethought and perspicacity, with all his great powers concentrated on the task in hand without a thought of self. A total abstainer and non-smoker, his only indulgence was a little jam with his tea; and his relaxation out of the line was a game of chess. A great man.’ And again: ‘By common consent he was the most distinguished combatant officer of the London O.T.C. before World War I: in that war he became a legendary figure. His bravery was outstanding. . . . We have lost a wonderful man. I am proud to have been a student at the same college and a brother officer in the O.T.C.’ It is said that Richardson was recommended for a Victoria Cross on account of the action of 21 March, and although this did not go through he received mention in despatches. From tributes of his many friends we gain the picture of a man big in mind and spirit, destined for great things, mentally and spiritually equipped to undertake any task or responsibility, a man in whom one could have absolute faith: ‘the only thing he pushed himself forward for was work and not rewards’.

Few men could have survived the severe wounds and consequent operations which he suffered. During most of the session 1924-5 Richardson was absent from Swansea as a result of an incredibly severe operation carried out by Sauerbruck in Munich for the removal of the bullet from his lung. Although the operation itself was successful the strain of it left a legacy of leukaemia, a progressive disease, which virtually turned him into an invalid for the rest of his days. It was then that his true quality was displayed, in the singleness of purpose with which he turned his immense vitality to the channels of teaching and research. Long hours spent resting on his bed by the open window were employed first in reading every single paper and work on modern algebra and its sources on which he could lay his hand, and then in applying this extensive knowledge to opening new fields of research. His letters, in later years, from Cape Town were full of mathematics and of comments on current affairs, with little mention of what he was suffering, but

with constant allusions to the abilities of his medical adviser, and once the typical remark 'Even here I have spent nearly a year in doing statistical work on Nutrition for my medical adviser and friend, Professor Brock', who had made a study of malnutrition among the natives of Central Africa for U.N.O.

At the opening of the session of 1920-21 when Richardson entered upon the duties of his Chair at Swansea his senior lecturer was Dr John Marshall of St Andrews and Cambridge. A staff of three conducted classes in mathematics for some sixty of the eighty-nine students of the newly founded college, first, by a temporary arrangement, at the Technical College until Singleton Abbey was ready for occupation, and later, when it was completed, at the Arts Building. Richardson proceeded steadily with the organization of academic studies and the administration of what, from the start, was the largest department of the college. He contrived so to arrange matters as not to be subjected to undue physical strain, and the fact that he was able to survive and prosecute research for thirty years is proof of the soundness of his judgment in remaining at Swansea. Shortly after his appointment he was offered the post of Commandant at Cranwell—a post after his own heart—which he wisely declined. The main cause, however, of his success in keeping the leukaemia at bay for so long lies in the devoted nursing of his wife. During the summer of 1922 Richardson married Dr Margaret Harris, a member of the Modern Languages Department at Swansea, and an expert on German linguistics. While in Swansea the Richardsons were noted for the quality of their hospitality: from South Africa they continued their splendid tradition of hospitality by sending out a stream of food parcels during the war to many of their friends whom they thought might be in difficulty, particularly those with children. They were both fond of children and many of the members of staff with young families found in Mrs Richardson a fairy godmother. In the last phase in South Africa they cherished more than any other news, that of the children they had known: it was this more than anything else that helped them to bear their homesickness. From Swansea they occasionally spent a winter or summer vacation at Madeira or the Italian Lakes in the hopes of improving his health: twice they returned from Cape Town to Britain in the summers after the war, but on each occasion it proved to be a strain for Richardson.

'For me', writes his wife, 'his essential quality was his goodness which was innate: he was free from all pettiness and meanness and was entirely guileless. The nobility of his character was apparent in his manner, bearing and expression. He was very dearly loved, and I think it was his goodness that attracted people to him and inspired such confidence. People liked to be with him because of his geniality, cheeriness and kindly sense of humour. . . . I cannot imagine him ever having shown fear however great the danger he was facing. I myself know more than anyone else with what noble but unassuming fortitude he bore his ill-health, the ups and downs of which he regarded as being "all in the day's work" . . . He had a great capacity for

enjoyment . . . few men *expected* less for themselves of life than he did and few men thought less of self. He had the gift of comforting and encouraging those in trouble. I think his greatness of spirit is shown by the rare happiness which was his in spite of 36 years of ill health and by the brimming measure of happiness he gave others. His life was a triumph: he was an example and an inspiration while he lived and he still is.'

He was a home-loving man: one of his chief hobbies while the state of his health still allowed him to live in Great Britain was the cultivation of fruit trees, and he loved to wander up and down the garden path and gaze with pride on his cherished cordons. He was an avid reader: works on military history were of great interest to him; in particular the accounts of the military tactics of Wellington. He enjoyed books of adventure and travel; he appreciated beautiful scenery.

Richardson went to Swansea in 1920 as a specialist in hydromechanics, and upon his appointment applied his energies to the moment problem, but without attaining the success he had hoped for. When shortly afterwards his colleague Dr Marshall left to become Reader in Applied Mathematics at Bedford College he was succeeded by Dr Paul Dienes, to whom Richardson entrusted the task of building up an honours and post graduate school in analysis, a step which left him free to follow his bent. His four earliest publications had been in analysis; they were followed by two (5, 6)* on hydro-mechanics: he now turned his attention to regions of modern algebra which were largely unexplored. Very soon he created an honours school of algebra which was at the time unique. Two of his students occupy Chairs of Mathematics in the University of Wales; R. Wilson, who had not graduated when first appointed to the staff, has succeeded him at Swansea, and D. E. Littlewood who came to him from school is now in the Chair at Bangor. Soon after his first arrival at Swansea Richardson planned a research school. In 1922 his staff had been increased to four by the addition of two assistant lecturers, one of whom was shared with the Department of Physics, and at that figure it remained until 1930. During these first few years he welcomed a number of postgraduate students, mostly members of staffs of local schools, some of whom were successful in obtaining the degree of M.Sc.: with this policy he prepared the way for the future honours courses with their concomitant postgraduate studies. He built a tradition of good teaching and played a great part in the early development of the college, in which he was ably supported by the late Professor E. J. Evans, then Head of the Department of Physics. Evans and Richardson had been fellow students at the Imperial College, and their friendship cemented the close associations between the Departments of Mathematics and Physics which still continues. On one occasion, at an end of term meeting of examiners, Evans turned to him and said, 'Richardson, there is something wrong here. One of my men has got 70 in physics and nothing in mathematics.' Richardson was no

* The numbers in brackets refer to the numbered bibliography at the end of this notice.

compromiser, but he felt that something was expected of him, so he made the great concession: 'Well, Evans, I will say this; he was the best of those that got 0.'

He was held in high esteem by his colleagues on the Senate and was ready to stand firm in his convictions however unpopular or novel they were. In his attitude towards pass students one gathers that his purposes and ideals were not fully understood. In his early days he had approached mathematics as an engineering student: he hoped now to invigorate the mathematical teaching for engineers when he devised a beautiful and quite modern intermediate course with most interesting pieces of algebra in it. After a struggle his liberal views were vindicated on the Senate, but he was unable to go further and put them into practice owing to his health: he never taught large classes again except at a critical period when his right-hand man was ill. He had a preference for an honours school in pure mathematics, with applied mathematics carried only to the first year of the honours stage, rather than for a standard honours course in pure and applied mathematics wherein each subject is carried to a lower level than in the single honours subject. Wilson has remarked on his tremendous prevision in matters mathematical: 'Even before he left he was trying to interest me as an analyst in those ideas of algebraic topology which analysts of the advanced American school have since incorporated in their research studies. He was also deeply interested in the logical foundations of mathematics and possessed a detailed knowledge of lattice theory. Unfortunately I was more attracted to the ideas of the Scandinavian-Finnish school, the value of which he did not appreciate.' At Swansea he much preferred Forsyth's classical work on the theory of functions, with its emphasis on algebraic integrals and the Riemann surface, to the modern texts of Titchmarsh and others. Richardson, who had known Forsyth since Imperial College days, frequently spoke of what he owed to his teacher: the respect was mutual, and Forsyth made frequent visits to Swansea for the purpose of meeting and staying with Richardson. Perhaps it was Forsyth who first aroused in Richardson the sense of propriety in a mathematical investigation—of not merely gaining the result, but gaining it by the right treatment. He told me that Forsyth used to describe certain work as *compelling*—'a term he always applied to the Lie theory. Forsyth's instinct was true there, for the Lie theory and van Neumann's theory of group integration are intimately connected.' In a letter written shortly before the end of the war (1944) he wrote, 'I wonder what the future of the Universities will be. From what I gather they will be swamped with a mass of very poor material; . . . somehow I feel that hard work and true scholarship will be at a discount, and social science, the science of the village pump, etc., will take its place': and again, 'It looks too as if the post-war period will see a heavy demand for routine teaching and also a materialistic outlook which bodes ill for research.' Wilson has further written: 'What Richardson is most remembered for by his colleagues on the staff at Swansea is his great integrity of character. His standards of morality and conduct were of the highest order. He was generous

by nature, but managed to conceal his private benefactions from all but those few who knew him intimately . . . Of his staff he expected allegiance to the high standards that he set himself, and he always stood by them in their difficulties. It is not easy to estimate how much he achieved in those early days by his example, but it undoubtedly had a great effect.'

The papers which Richardson published on his research work in algebra are of two kinds; those which deal with technique and those on the abstract theory. His general outlook was that such and such problems have long ago been worked out in ordinary numbers, but presumably have their counterpart if ordinary are replaced by hypercomplex numbers, or ordinary algebra is replaced by non-commutative algebra such as that of quaternions. The paper (10) of 1926 is a striking example of this. In it he showed how to solve general linear equations such as

$$\sum a_s x b_s = \alpha$$

for x in a division algebra to which the coefficients a_s , b_s and the constant term α belong. In a non-commutative system the coefficients a_s and b_s are separated by the x , since $x b_s \neq b_s x$ in general: the equation is two-sided, being one-sided only if all the coefficients appear only on the left (or right) of x . The difficulties consist first in devising a suitable generalization of determinants in order to solve a system of left-sided equations, and next in applying the method to a two-sided equation. Both difficulties were overcome, and a solution was given analogous to Cramer's rule involving the quotient of two determinants. When the elements are non-commutative the determinant of even a two rowed matrix

$$\begin{bmatrix} a & b \\ c & d \end{bmatrix}$$

is not quite obvious: to have begun with the form $ad-bc$ would have led nowhere. Instead, Richardson used what he called a left-hand determinant $da-dbd^{-1}c$, and a right-hand determinant $ad-cd^{-1}bd$, the element d acting as a pivot, but one which could not, of course, be cancelled as in ordinary algebra. He distinguished the left- and right-handed determinants, by an appropriate notation such as

$$\left\| \begin{array}{cc} a & b \\ c & d \end{array} \right\| \quad \text{and} \quad \left| \begin{array}{cc} a & b \\ c & d \end{array} \right|,$$

and he extended the definition inductively to the determinants of order n : a non zero pivot a_{sn} was chosen from the n th column of the s th row in order to define a determinant of order n in terms of those of order $n-1$; and it led to the solution of linear equations. A surprising number of properties of ordinary determinants survived with little modification; and here the interest centred upon the type of modification adopted. By surrendering the ordinary definition of polynomial and admitting terms such as the above $dbd^{-1}c$, where the factor d^{-1} could have been removed had the algebra been commutative, he

could welcome the new determinants as polynomials in their constituent elements. By so doing he was able to retain the important theorem that a determinant is irresoluble into factors which are polynomials in the elements.

Such treatment characterizes many of Richardson's early papers and the problems which he suggested in seminars to his research students—problems in the solution of quadratic equations in quaternions and the concomitants of polynomials in non-commutative algebra. One interesting result is the extension of Fermat's theorem $x^n \equiv x, \text{ mod } \mathcal{N}$: it became

$$x^2(x^{N(N^2-1)}-1) \equiv 0, \text{ mod } \mathcal{N},$$

when x was a real quaternion integer and $\mathcal{N}$ a prime positive integer (13).

The paper (15) on concomitants in non-commutative algebra is an interesting sequel to that on the solution of linear equations: in it he laid the foundation of an invariant theory of forms in variables which undergo linear substitution, the variables and the coefficients (right and left) of the forms, and the substitutions all belonging to an algebra of order n . Something akin to the classical theory of binary and higher forms was developed, but it involved two preliminary and novel features: first the effects of a change of basis from e_i to ϵ_i by linear equations $\epsilon_p = \sum \alpha_{pq} e_q$, and secondly the study of the case prior to binary forms—that of homogeneous linear transformations in *one* variable only, from x to X where

$$x = \sum_{p,q} l_{pq} e_p X e_q.$$

In it was preserved that pleasant feature of the ordinary binary theory of invariants, the build up of the general linear transformation from a succession of simpler transformations, or conversely the analysis of the general group by means of its subgroups. Orthogonal and diagonal substitutions were studied: invariants, particularly those of linear forms, were found. As in several other instances at this period the paper on concomitants (15) was the fruit of a happy collaboration with D. E. Littlewood, who was a pupil trained in his own school and then a beginner in research. Some work was handed over to Littlewood: 'He gave it to me saying that if I polished it and put it into shape we could publish it in collaboration. He would have been quite satisfied if I had done just that, but in the event I was able to add quite a significant contribution of my own which pleased him very greatly.' Littlewood had discovered a second type of invariant of a linear form that supplemented what Richardson had already found, and enlarged the scope of the enquiry.

There followed at this stage of collaboration an investigation that led to the important paper (16) on group characters and algebra, which opened the door for considerable advances during the last twenty years. The work centred on the symmetric group of order n , for which, as Frobenius has shown, there is a character table of integers set in p rows and columns, p being the number of partitions of n . It may be illustrated by the symmetric group of

the third order, with $n = 3$, $p = 3$. The six operations of this group may be represented by the six permutations

$$a_1a_2a_3; \quad a_1a_3a_2, \quad a_3a_2a_1, \quad a_2a_1a_3; \quad a_2a_3a_1, \quad a_3a_1a_2, \quad (1)$$

here arranged in three classes of conjugate permutations, the classes containing 1, 3 and 2 members respectively. But the same group has a character table of three rows and columns, the columns being headed by these same numbers, which denote the orders of the classes:

$$\begin{array}{ccc} 1 & 3 & 2 \\ \hline 1 & 1 & 1 \\ 2 & 0 & -1 \\ 1 & -1 & 1 \end{array} \quad (2)$$

Associated with any 3×3 matrix

$$\begin{bmatrix} a_{11} & a_{12} & a_{13} \\ a_{21} & a_{22} & a_{23} \\ a_{31} & a_{32} & a_{33} \end{bmatrix}$$

is a determinant $\Sigma \pm a_{1i}a_{2j}a_{3k}$ and a permanent $\Sigma + a_{1i}a_{2j}a_{3k}$ each summed for the suffixes i, j, k according to the scheme (1), whereas the coefficients, all of which are ± 1 , follow precisely the particulars of the third and the first rows of the table (2). This suggested to Richardson an intermediate function

$$2a_{11}a_{22}a_{33} - a_{12}a_{23}a_{31} - a_{13}a_{21}a_{32},$$

on the basis of the second row, and in general a set of p functions of the elements of a $p \times p$ matrix, starting with the permanent and ending with the determinant. These functions he called *immanents* (the word was suggested by Forsyth). The number p was not arbitrary but was the number of partitions of n ; for such is the number of rows and columns of the character table for the symmetric group of order $n!$ (Each row of the table is associated with a particular partition $\{\lambda\}$ of n ; and so, too, is each column, but for a different reason.) With this ingenious idea in mind he laboriously calculated the immanents of various matrices, particularly those of the determinants expressing one symmetric function in terms of another type: all these calculations he handed over to D. E. Littlewood for perusal; and after further consideration they found that when all these expressions were put in terms of the x_i , of which they were symmetric functions, then the immanents were recognized to be the bialternants

$$| x_s^{\lambda_i + n - t} | \div | x_s^{n - t} |$$

where $s, t = 1, 2, \dots, n$, $\lambda_1 + \lambda_2 + \dots = n$, $\lambda_1 \geq \lambda_2 \geq \dots$. This occurrence of the λ_i 's in the exponents of the x_s 's gave an added interest to the partition $\{\lambda\} = \{\lambda_1 \lambda_2 \dots\}$ of n , for already the immanent was related to such a partition through its definition from a row of the character table. At this stage Richardson read conscientiously through the literature,

including Muir's *History of determinants*, seeking every reference to symmetric functions. This brought to prominence another link between the immanent and the character table. For the immanent, being a bialternant and therefore a symmetric function of the x_i , was expressible rationally and integrally in terms of the elementary power sums $s_r = \Sigma x_i^r$, a feature which led to relations such as

$$\begin{aligned} 3!\{3\} &= s_1^3 + 3s_1s_2 + 2s_3 = 3!\{\overline{111}\} \\ 3!\{21\} &= 2s_1^3 - 2s_3 = 3!\{\overline{21}\} \\ 3!\{111\} &= s_1^3 - 3s_1s_2 + 2s_3 = 3!\{\overline{3}\} \end{aligned}$$

where the character table is evident in the coefficients, which are weighted with the orders 1, 3, 2 of the classes. The immanent is denoted by $\{\lambda_1\lambda_2 \dots\}$ a partition, or by $\{\mu_1\mu_2 \dots\}$ a conjugate partition, of n (here $n = 3$). The first $\{3\}$ is h_3 or equally well a_{111} (in Macmahon's notation): the last $\{111\}$ is h_{111} or equally well a_3 . Such a connexion is suggested by the suffixes upon the principle diagonal of the determinant

$$\begin{vmatrix} h_{\lambda_1 - s + t} \end{vmatrix}$$

which is another form of the bialternant or immanent. Richardson saw that the foundations of the whole matter were laid years ago by Jacobi. From time to time he told me by letter of his latest thoughts on algebra, and on hearing of this I referred him, in my reply, to Aitken's recent work on symmetric functions and A. Young's then recently published paper on standard tableaux. I think I also mentioned Schur's classical dissertation on invariant matrices ('Über einer Klasse von Matrizen' Berlin (1901)), for he eagerly sought it out and eventually found a copy in the college library that he had bought several years before, but had completely forgotten.

There was at first perhaps a feeling of disappointment for the collaborators that Schur had anticipated them to some extent, but the new insight, that these immanents and S -functions (as they came to be called in honour of Schur their originator) were related to invariant matrices was indeed worth the loss of precedence: and, as Littlewood has said, 'It was quite marvellous to see the intricate theory taking shape under our hands.' Richardson and Littlewood continued their joint work for a few years on the problems which arose from these functions. Applications developed, which have led to a vigorous school of algebraists working out the implications of group characters for invariant theory, who have gone a long way towards fully answering the question, mentioned above, regarding symbolic and ordinary methods, which had originated the investigation. Richardson, was a pioneer rather than a consolidator; and now that the scheme was fully launched and in very competent hands, his interests turned towards more abstract theory. Despite his illness he was constantly looking forward to the problems that awaited solution and to further generalizations. 'The next century', he wrote, 'must deal with the most general kinds of algebras in which the connecting

relations are less restricted: for example, non-associative algebras and the theory of partially ordered algebras.'

During his later years at Swansea he was at work on a book, the contents of which may perhaps be gathered from the short note that appeared in the *Mathematical Gazette* (22): it represented his attitude at the time, and sketched his course of lectures and the treatment for the book. He was coming more and more to the conclusion that many of our ordinary theorems are really of much wider extent. For example, the group characters are not really intrinsic to groups—they are invariants associated with equivalence relations when these combine under a law of multiplication suitably postulated. In other words they have really nothing to do with any multiplication laws postulated for the elements: what is essential is that the set should be well ordered. 'I am still making slow progress with the book', he wrote in 1938, 'and have found it necessary to write and rewrite the earlier chapters until now the work has taken on almost the character of research.'

Certain ideas are common to many different special branches of mathematics, such as theories of congruence, measure, distance and dimension, of classes and genera, ideals and rings, groups and their characters, of valuations and modular functions in general algebras. These theories have some or all of the following ideas in common:

- (a) A formal mathematical system A is defined; e.g. a group.
- (b) General relations determine subsystems of A ; e.g. equivalence relations.
- (c) These subsystems of A are regarded as instances of a formal system B ; e.g. the class-ring of which the elements are the classes of conjugate elements of a group.
- (d) Correspondences are established between B and other systems such as the integers, algebraic numbers, A itself.
- (e) In many instances such correspondences are homomorphic.

Richardson aimed at discovering an abstract theory governing these considerations (20). As, however, little was known about general systems he had to discover the necessary technique and construct examples to serve as pointers to his goal. Mathematics throughout has been greatly concerned with binary relations (addition $a+b$, multiplication $a \times b$, the combination of two elements in a group to make a third element), and such relations undergo generalization. By suitable modifications of the binary nature of the relations between elements and of the laws of association, distribution and cancellation, he succeeded in establishing homographic correspondences between systems (mostly non-associative) and certain subsets of the set of all equivalence relations over the systems, while at the same time retaining many of the more important theorems of the special branches (21, 24, 25). In his abstract work he never lost touch with reality: in a characteristic paper (20) he even enlisted the theory of probability. 'Have you seen anywhere' he wrote in 1938, 'an estimate of the *strength* of a postulate or the *extent* of a

theorem? I use the theory of probability to define these. One interesting result is that the postulate that a finite number system contains an idempotent element is about e^{-1} . For number systems containing only 4 elements I have calculated the extent of the theorems of Lagrange and Sylow.' The paper was a preliminary survey, as he put it, of the extent to which certain theorems are likely to be true in an arbitrary multiplicative system.

In the course of his general investigations a number of special results came to light. For instance he showed how to calculate by purely rational methods p commutative rational matrices (each linear in the variables and of order equal to that of the Galois group) which are factors of the binary p -ic (19), and in (25) he extended his methods to the general m -ary p -ic. In 1937 Temple had interpreted the Clebsch Aronhold symbols as those of a nilpotent algebra, a step which led to a renewed interest in them. Richardson appears to have made an independent discovery; for, commenting on his own factorization, he remarked (1942), 'It follows then that the symbolic methods are really calculations with suitable matrices. I have suspected this for a long while and especially its connexion with tensor algebra. . . . My new method is direct—I actually factorize the form. Incidentally this ought to open up a new idea in the theory of algebraic functions. It also enlarges the notion of group characters and pseudo-representations.' By such a representation he meant a double set of commuting matrices C_i and C'_i satisfying the relation

$$(C_i C_j - \sum c_{ijk} C_k) (C'_i C'_j - \sum c_{ijk} C'_k) = 0$$

summed for the self-conjugate classes of the groups (25).

By expressing the conditions that the matrix factors of a form be singular he showed that different factorizations over a domain of integrity correspond to different classes and forms of that domain. This led him to new theorems relating to the composition of forms, and to the solutions of the corresponding Diophantine equations; and it gave new proofs of some of L. E. Dickson's theorems on ternary forms, and related them to generalized quaternions and the double theta functions.

This work was done in South Africa amid increasing ill health. Soon after his arrival there he devoted nearly a year to the statistical work already mentioned. Periods of sustained work became interrupted, yet he continued his research. The Young tableaux had first come to his notice through the properties of group characters: and they continued to fascinate him. He saw in the PN relations which Dr A. Young associated with the tableaux a refinement of immanents, and he set out to make the tableaux of order n into a structure, first defining order and equivalence. 'We can then define union and cross-cut as for equivalence relations. Unfortunately, or, of course, fortunately, the union of two Young tables according to this rule need not be a Young table. . . . I can get over this difficulty (in the cases examined) by using a Hasse diagram.' The result up to $n = 5$ was a complemented Dedekind structure or, according to Birkhoff, a Projective geometry. But it

broke down at $n = 6$, 'the union of two Young tables not being unique. I must try again.' This was written in 1946 when he was still helping Professor L. C. Young at the University of Cape Town. 'I hope to return to this problem later on, but now some of Young's post-graduates return from their holidays and I shall be kept busy with them until November. They come to our flat, for I have been unable to get to the University for over two years.'

His main interest was, however, the arithmetic of forms, and he produced a series of papers on the composition first of the quadratic and later of the cubic form: in both these cases he appears to have made some remarkable advances. The first (26) was attained by an ingenious use of matrices, starting with an unexpected addition of a skew symmetric matrix into the formula $x'Ax$ for a quadratic form, then proceeding with commuting and scalar matrices, and ending with a battery of skew determinants and of Pfaffians, directed with skill and aimed at reducing to quadratics any factors of higher degree in the sets of parameters which they involved. Thus he factorized $f(x)$, any quadratic form in n indeterminates x_i over a commutative and associative ring $\mathcal{R}$, and obtained a resolution

$$f(x) = u(\alpha') \cdot v(\alpha),$$

where each of u and v is a quadratic in its parameters α'_i and α_i , of order 2^{n-1} over $\mathcal{R}$, and where x is bilinear in α and α' over $\mathcal{R}$. Though this formula was not identically true as regards α and α' , yet it was found to be true for an infinite module of elements in $\mathcal{R}$ which satisfy certain quadratic and bilinear relations depending on n , but independent of f . As a sequel to this general investigation he considered the case, of historic interest since the time of Fermat, in which

$$f(x) = f(\alpha') \cdot f(\alpha).$$

This result is known to be an identity, when x is a suitable bilinear combination of α' and α over $\mathcal{R}$, if and only if $n = 1, 2, 4$ or 8 . Richardson worked out the case when $n = 16$ (and indicated that it would extend to the case when $n = 32$), again obtaining a result which was no longer an identity, but true for an infinite module of elements (27). As a corollary he deduced a theorem of A. Hurwitz (*Math. Annalen*, **88** (1923) 1), that the product of any sum of 5 squares and any sum of 16 squares is a sum of 16 squares identically and bilinearly over $\mathcal{R}$: and a further corollary, that if $u_1(x)$ be a sum of 16 squares then there exist three further sums of 16 squares, u_2, u_3, u_4 such that

$$u_4(x) = u_1(\alpha)u_2(\alpha')u_3(\alpha'')$$

where x is trilinear in $\alpha, \alpha', \alpha''$ which lie in a specific module over $\mathcal{R}$. And among further results is the curious fact that the product of any two sums each of 16 squares over $\mathcal{R}$ is identically and bilinearly equal to the sum of 71 squares minus the sum of 36 squares.*

* Richardson sent the paper to America in 1944 but it has not been published.

The key to the general method was first to resolve the $m \times m$ scalar matrix $f(x)I$ (where $m = 2^n$) into two commuting matrices, then to deduce a corresponding factorization of $(f(x) - uv)I$, for arbitrary u and v , and then to express the fact that, for values of the x_i which make $f - uv$ vanish, the matrices must be singular. When f permitted composition into the quadratics u and v , in the above sense, he found that *each* of these quadratics permitted composition into the other two. The work put into a new perspective the long history of quadratic forms from the time of Gauss onwards.

He also wrote two short papers on cubic forms, the first (28) in 1947, and four years later a sequel (29). 'What does astonish me,' he said, 'is the vast gulf separating the quadratic case from the cubic': there was no starting point for the cubic comparable to the convenient matrix product $x'Ax$ of the quadratic. None the less he made a surprisingly effective advance into a region which is still largely unexplored. Using ternary row-vectors x, α, β, γ (where $x = [x_1, x_2, x_3]$) he resolved a general ternary cubic $f(x)$, obtaining

$$f(x) = a(\alpha) \cdot \phi(\beta, \alpha),$$

where $a(\alpha)$ is a ternary cubic in the α_i , and $\phi(\beta, \alpha)$ a bi-cubic in the β_i and α_i , over $\mathcal{R}$. It was done by starting with a 3×3 matrix $F(x)$ whose nine elements were linear forms in the x_i , and whose determinant was $f(x)$, then deducing a kind of transposed matrix $A(\alpha)$, linear in the α_i and defined by the matrix equation $x \cdot A(\alpha) = \alpha \cdot F(x)$, whose determinant was $a(\alpha)$ and whose adjoint matrix was $A^*(\alpha)$, and finally proving the Lemma:

If $x \cdot A(\alpha) = \alpha \cdot F(x)$ and if $x = \beta \cdot A^*(\alpha)$ then $f(x) = a(\alpha) \cdot \phi(\beta, \alpha)$.

It led to some elegant relations of reciprocity among the x, α, β and further parameters, and among the factors of composition. Since these identities hold for general $F(x)$, several infinite sets of solutions of $f(x) = uv$ may be found, but it seems improbable that all solutions arise thus: so ended his short but fascinating paper, written early in 1951 in an interval of renewed strength after a gap of three years of increasing physical weakness. He told me by letter that $\phi(\beta, \alpha)$ may or may not factorize into $a(\alpha)$ and a cubic in β , which is so for a cubic due to Legendre associated with the binary cubic. He conjectured that the lemma would be true for n variables with

$$f(x) = (a(\alpha))^s \cdot \phi(\beta, \alpha)$$

for some power $s = 1, 2, \dots$, or $(n-1)$, f being an n -ary n -ic: he had found the factor ϕ in a long hard way; he suggested, too, that the theory of invariance was a special case and that 'there seems also a trace of Schur's invariant matrices. Please help.' In reply I sent him a proof of the lemma by symbolic methods (he had hoped that one might have seen the matter in a flash!): some months later he wrote once more: 'my own interests lie rather in a set of interchangeable relations between the various cubics, with a view to extending the theory of composition to cubic forms. However it is certain that I shall never be able to do this. I have had the above results for some

years and my letter to you arose from a brief spell when I was able to work to write up a short account.' He continued to ponder over and to note down his research until the end.

Richardson died on 4 November 1954: a leader by inspiration and action, who gave of his best at the call of his Country and thereafter in his home and college life. He has an honoured place among algebraists in the succession of Macmahon, Young and Wedderburn: a great man has passed on.

It is not possible to thank all those who have provided material for this notice, but above all my thanks are due to his wife, and to his colleagues Professor R. Wilson and Professor D. E. Littlewood.

H. W. TURNBULL

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Diego Salazar

REDCLIFFE NATHAN SALAMAN

1874-1955

REDCLIFFE NATHAN SALAMAN was born on 12 September 1874; he was ninth of a family of fifteen, of whom fourteen, seven boys and seven girls, survived as adults. The family home at that time was Redcliffe Gardens, London, S.W., and this was the origin of Salaman's first name. His father was Myer Salaman who was born in 1835 and died in 1896; his mother Sarah Salaman was born in 1845 and she died in 1931.

Salaman's great grandfather was Aaron Solomon, a well-to-do merchant who lived in a country house in Edmonton and who manufactured and dealt in Leghorn hats. His grandfather, Isaac, was the youngest son of Aaron, born in 1790; he left home for some reason unknown and joined the Navy as a common sailor. He was bought out by distant relatives and returning home changed his name to Salaman.

Redcliffe Salaman was twice married, his first wife, Nina, whom he married in 1901, was the daughter of Arthur and Louisa Davis. Her father, an engineer, was also a distinguished scholar who specialized on the Massoretic Text of the Bible.

Nina herself was an outstanding scholar of the Spanish Hebrew period; she died in 1925. By his first wife Salaman had six children, the eldest, Myer, is Pathologist at the London Hospital, Cancer Research. The second son, Arthur, is a doctor in general practice and a twin brother, Edward, died at the age of 9. The fourth son, Raphael, is an engineer, and of the two daughters one, Ruth, is an artist and the other, Esther, a singer.

Salaman felt deeply the death of his wife, who was an extremely beautiful woman, and for a time his life was disorganized. He was lonely and had no one to run his fine house at Barley. However, in 1926 he met and married secondly, Gertrude, daughter of Ernest and Henrietta Lowy, and for nearly thirty years she managed his home and gave him her devoted love and care.

Salaman's grandchildren were a great joy to him and he frequently had them to stay with him at Barley. [He died at his home there on 12 June 1955.

School and University

The parents of Redcliffe Salaman were well-to-do and he was brought up in a town and a country house. As a child he was greatly influenced by his old nurse for whom he had a great affection. In the town home, the nursery and 'downstairs' were two distinct worlds, but in the country home, Wentworth House, the distinction was less pronounced.

From the age of 6 to 10 years Salaman had a governess, but from 10 to 12 he attended Sam Bewsher's Preparatory School for St Paul's School, later known as Collet House. At the age of 12 Salaman went to St Paul's where he was on the Classical Side till he passed the London Matriculation at the age of 16. He was not happy on the Classical Side and took little interest in any subject. Both Greek and Latin were serious obstacles to him, chiefly, he used to say, owing to the dull method of teaching, because he was keenly interested in literature of all kinds. He, therefore, changed over to the Science Side, where, under the influence of Thomas the Biology Master, a lively interest was awakened.

He became Head Boy on the Science Side and in his 18th year passed his Preliminary M.B. London, and obtained a Leaving Scholarship from St Paul's, and a scholarship at Trinity Hall, Cambridge.

He entered Trinity Hall as a scholar in October 1893, and in 1896 took the Natural Science Tripos, with a First Class in Physiology, Zoology and Chemistry.

Among Salaman's teachers were Walter Gaskell, who was his tutor and adviser and subsequently a close friend, Professor Michael Foster, whose lectures in physiology he found fascinating and Professor Alexander MacAlister, whom he knew personally and greatly admired.

He qualified B.A. in 1896, B.Ch. in 1900, M.R.C.S. and L.R.C.P. in 1900, M.B. in 1901 and M.A. and M.D. in 1904.

Postgraduate studies and scientific work

Salaman studied medicine at the London Hospital from October 1896 until he qualified. During this period he spent six months in the Pathological Laboratory, Claybury, studying histology under the guidance of Henry Head and the Director, Sir Frederick Nott.

In 1901-1902 he went to Wurtzburg to study morbid anatomy and pathology under Professor Borst and under Professor Benda in Berlin for twelve months. Later Salaman was appointed Director of the Pathological Institute at the London Hospital. He held the appointment till 1904 when an event occurred which changed the whole course of his life. He developed tuberculosis of the lung and had to give up all his medical work. He spent six months in Switzerland and about two years in the country making a recovery. He then bought a fine house, Homestall, in Barley, near Royston, Herts., where he was to live for more than fifty years.

About this time he became interested in genetics under the guidance of William Bateson, who was his friend. He selected the potato as a medium of research and worked in and around this subject for the rest of his life. All his genetical research was carried out privately in his garden at Barley. Here, also, was discovered in 1908, the genetic resistance to potato blight, *Phytophthora infestans*; this was repeated in 1914, and the work was enlarged and extended at Cambridge. In this work the wild relative of the potato plant, the Mexican species, *Solanum demissum*, was used to introduce the factor of

resistance to potato blight. Salaman was the first to do this and *S. demissum* is still used as one of the main sources of resistant genes.

As very little modern research had been applied to the potato prior to 1906 it was not long before Salaman's work became known and his participation was invited in a number of cognate directions. Perhaps the most fruitful of these was his Chairmanship of the Potato Synonym Committee, whose work entirely cleared up the vexed question of varietal differences in the potato, and put a stop to the very common practice of marketing old varieties under new names at enhanced prices.

The results of Salaman's work at Barley were published in 1926 in a book *Potato varieties*. In 1926, also, he interested his friend, Sir Daniel Hall, and the Ministry of Agriculture in the foundation of an institute at Cambridge for the investigation of plant virus diseases with especial reference to the potato. This was known as the Potato Virus Research Institute and was the forerunner of the present Virus Research Unit of the Agricultural Research Council. Salaman was the Director of the first-named Institute until his retirement under the age limit in 1939.

Part of Salaman's work during this period was to try and build up a large stock of virus-free potatoes and gradually to feed these back into the industry, and thus to raise the standard of the commercial potato stocks which at that time were becoming saturated with virus diseases. In conjunction with the late Professor Paul Murphy of Dublin, a nucleus stock of virus-free potato seed was built up by means of careful testing and selection, and these were further multiplied in the insect-proof glasshouses at Cambridge. The next step was to try and build up these stocks on a commercial scale and with this aim in view Salaman visited the Hebrides in 1934 to study the possibility of growing large quantities of seed potatoes in isolation from all insect-borne potato viruses. His findings were embodied in his 'Report on a scheme for raising virus-free potato stocks'.

The present large scale production of virus-free potato seed now being carried out by the National Institute of Agricultural Botany is a direct outcome of Salaman's work at Cambridge; incidentally, he was elected an Honorary Fellow of the National Institute of Agricultural Botany in 1952.

This idea of raising virus-free stocks of potatoes with a view to increasing the yield and quantity of the crop generally has now been copied in many other countries. The technique of testing the virus content of the potato plant by inoculation to indicator plants developed at Cambridge by Salaman and his co-workers is still used, together with the more modern and rapid technique of the precipitation of antisera.

One of the methods used by Salaman for testing the virus content of potato plants was to graft scions from the plant under test to numbers of potato plants of different varieties. It was during these routine graftings, carried out with the help of R. H. Le Pelley, that the interesting discovery was made of the latent virus, called Paracrinkle virus, present in all plants of the potato variety King Edward. This is one of the puzzles of the plant virus world and

its invariable presence in a particular variety of potato is one of the strongest pieces of circumstantial evidence for the endogenous origin of some plant viruses. This discovery acted as a stimulus for further investigation of latency in plant viruses.

Salaman was also interested in the study of the ubiquitous virus, known the world over as potato virus X, and he was one of the first to demonstrate, by means of strains of this virus, the kind of acquired immunity which develops in plants infected with one virus against the entry of another related virus or virus strain.

By selective methods of mechanical transmission he built up a collection of strains of virus X, varying from the most virulent form to one which was completely symptomless. These viruses have been greatly used by subsequent workers on this subject.

Salaman's interest, however, was more in the potato than in viruses and in addition to his genetical work he spent much time studying the influence exerted by the potato, as a cheap and easily produced food, on the structure of society, especially in the eighteenth and nineteenth centuries. These studies were incorporated in his book *The history and social influence of the potato*, published by the Cambridge University Press in 1949.

Salaman was elected F.R.S. in 1935, and of all his honours, this he valued most. He took a great interest in the Society and attended many meetings.

War service

On the outbreak of war in 1914, Salaman was appointed Recruiting Officer for the district. In March 1915 he joined the R.A.M.C. as Lieutenant and was later promoted Captain. He served in England till 1917, when he went as Medical Officer to the Jewish Regiment, 39th Royal Fusiliers, and served in Egypt and Palestine. He was mentioned in despatches.

In the Second World War he was Section Commander in the Home Guard. He also acted as Billeting Officer, Voluntary Food Organizer and Member of the Potato Advisory Committee, Ministry of Agriculture.

Local and communal activities

Salaman was a man of culture and wide interests, he came to maturity before the age of specialization, when a man who wishes to make his mark in the scientific world must give more and more attention to less and less.

He had many local activities; he was J.P. County of Herts., 1907; Member of the Quarter Sessions Appeal Committee; Commissioner of Taxes; Vice-President, Royston and District Hospital; Chairman of the Barley Parish Trustees and Chairman of the Parish Council.

Salaman was a magistrate for 43 years and was Chairman of the Bench for 23 years before he retired in 1950. On that occasion his fellow magistrates presented him with an illuminated address.

Among his many communal activities may be mentioned, Late President, Union of Jewish Literary Societies; Late President, Jewish Historical Society;

Late Chairman, Jewish Committee for Relief Abroad; Governor of the Hebrew University, Jerusalem; Trustee, Jews' College; Founder and Chairman, Israel Zangwill Memorial Fund; and Member of Council and Allocation Committee, Society for Protection of Science and Learning. Of these activities two were especially dear to him. He was deeply interested in the Hebrew University of Jerusalem and did much to help it in its earlier years. He made an annual visit to Jerusalem for the meetings of its Board of Governors. The Hebrew University acknowledged this by making him an Honorary Fellow. Furthermore, it is proposed that there should be a memorial to him and it is suggested that it should take the form of a corner of the Botanical Garden of the Hebrew University with a plot for the study of the potato plant.

His second great communal interest was the Society for the Protection of Science and Learning, and he gave himself whole-heartedly to this cause. He did much to give new opportunities to displaced scientists and scholars, and personally helped many to start again.

Salaman was at all times a genial and kindly man and he showed these qualities to great advantage when acting as host at his home to members of the Duodecimo Club. To these meetings he often invited his friends, including the writer of this memoir.

In 1955, just before his death, Salaman was elected to an Honorary Fellowship of his old college at Cambridge, Trinity Hall. This gave him great pleasure; the pity is that the award was not made earlier so that he could have had some time to enjoy his Fellowship.

In 1955 also, he was elected an Honorary Member of the National Association of Seed Potato Merchants in recognition of his services as Chairman of the Potato and Potato Synonym Committees. He was also awarded by the same Association the 'Haigh' Trophy, 'for outstanding meritorious service to the Seed Potato Trade in general and to the Association in particular'. This award would have been very welcome to him, but it is doubtful if the announcement came in time for him to hear of it.

KENNETH M. SMITH

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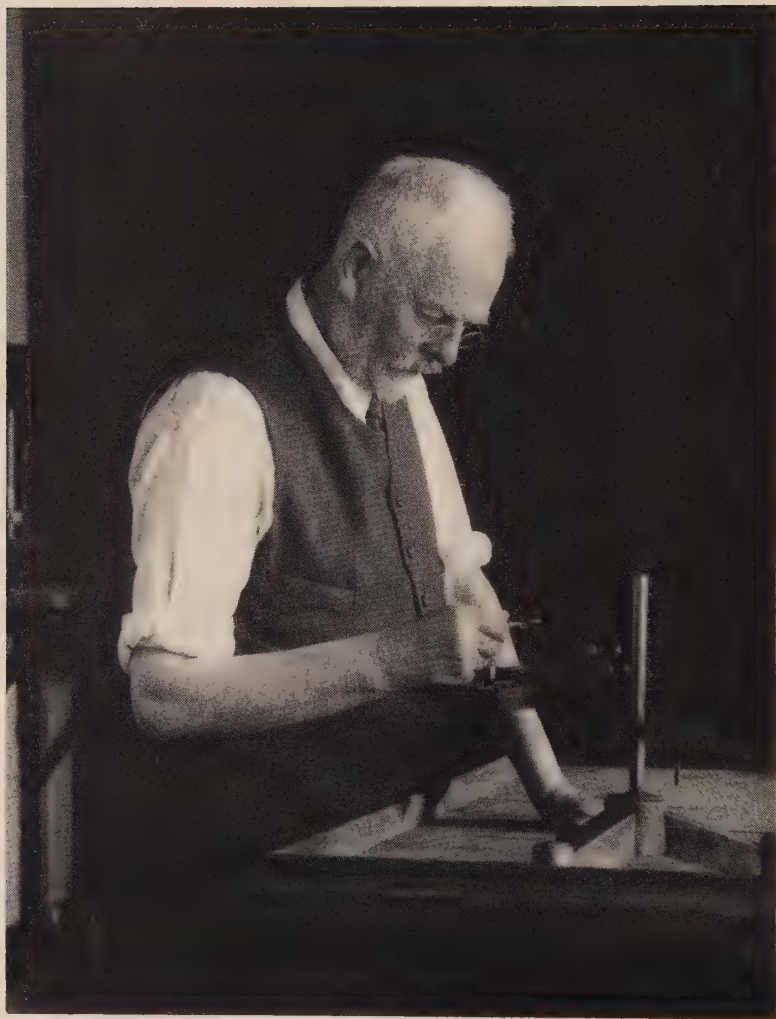
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G. F. C. Searle

GEORGE FREDERICK CHARLES SEARLE

1864-1954

GEORGE FREDERICK CHARLES SEARLE was born on 3 December 1864 at the village of Oakington in Cambridgeshire, of which his father, the Rev. W. G. Searle, was vicar. He was the eldest of five children, three boys and two girls. His schooling was limited to six days at the village school, otherwise he was taught by his father, who though a Wrangler was more interested in languages, and taught him Hebrew as well as more usual subjects. G.F.C. retained enough Hebrew in later life to impress the custodian of a synagogue at Gibraltar. He taught himself the use of tools, which was a life-long pleasure. He constructed a dynamo while a boy. At the age of 14 he was taken by Clerk Maxwell round the Cavendish Laboratory, which deeply impressed him, and seems to have turned his thoughts to the direction of physics. He was a tall, well-built man of a little under six foot in height. As an undergraduate he gained a half-blue for cycling, then an important sport, and continued to bicycle for pleasure and utility to an advanced age, but he was not interested in games, nor did he greatly care for walking.

After a period of private coaching lasting about 18 months with a Mr Barrel, Searle came up to Cambridge with a foundation scholarship to Peterhouse. He read mathematics and was placed 28th Wrangler in the Tripos of 1887. He then studied for Part II of the Natural Sciences Tripos in which he took a second class next year.

In 1888 he began research at the Cavendish under J. J. Thomson, who had then been professor for four years, thus starting a connexion with the laboratory which lasted, with a short break in World War I, and another before World War II, for fifty-five years. In 1890 he was appointed University Demonstrator, and in 1900 University Lecturer, an office he held till 1935, coming back to his old laboratory in World War II, and finally retiring in 1947.

He was made a Fellow of the Society in 1905, and took a Cambridge Sc.D. in 1912.

His early research falls into several heads. First in time comes the work he did with Thomson on the ratio of the electrostatic and electromagnetic units of electricity, published in the *Philosophical Transactions* for 1890. The value obtained $2.9955 \times 10^{10} \text{ cm s}^{-1}$ agrees with modern values to 1 in 1000. The method was to calculate the capacity of a condenser in e.s.u. from its dimensions and determine it in e.m.u. by repeated charging and discharging when it was in one arm of a Wheatstone bridge. Searle took the observations.

The second line he took up was a more practical one, the testing of magnetic properties of iron and steel. He invented a magnetometer method, and later a ballistic one. This he developed, with the help of T. G. Bedford, into a means of measuring magnetic hysteresis ballistically, using an electro-dynamometer. In this instrument specimens of shapes that could conveniently be prepared could have their hysteresis quickly measured, and experiments were made on the variation of hysteresis with stress and other physical conditions. He also published papers dealing with the calculation of self and mutual inductances, and allied problems in applied mathematics.

A more important line of work is first described in a paper in the *Philosophical Transactions* of 1896, which deals with the motion of charged bodies, especially spheres and ellipsoids. In this work he was influenced partly by Thomson, whose original paper on the subject appeared in 1881, but more by Heaviside. Searle had reviewed a paper by Heaviside and pointed out an error which the latter was quick to acknowledge. This led to a friendship which lasted till Heaviside died. Searle had a great admiration for Heaviside's rather wayward genius, and was one of his few intimates. Searle followed up the 1896 paper with several others, and returned to the subject as late as 1942, but perhaps the most important results were in the original paper. He showed that in a moving conducting sphere the normal to the surface would not coincide in direction with the electric force at the point, but with a resultant compounded of this and what is now called the Lorentz force, though in fact it was introduced by Heaviside in 1889, three years before Lorentz's paper. This work is of importance as being on the line which led up to the special theory of relativity by way of the Fitzgerald-Lorentz contraction first put forward in 1892. Searle showed that the moving charged conductor which produces the same effect as a point charge at its centre is not a sphere as in electrostatics, but an oblate spheroid with its axis reduced in the direction of the velocity in the ratio $(1 - u^2/c^2)^{1/2} : 1$.

Searle's mathematics were perhaps more effective than elegant, but he used them well throughout his long life. His paper of 1946 on the location of interference fringes is remarkable for a man of 82. During World War I he worked at the Royal Aircraft Factory, now R.A.E., at Farnborough, and developed a recording accelerometer for use in aircraft on lines suggested by Lord Cherwell, then F. A. Lindemann.

'Searle's Class', as it was universally called, was the practical class taken by undergraduates reading for Part I of the Natural Sciences Tripos. It included, therefore, a large proportion, indeed a majority, whose primary interest lay in some other subject. In the 1920's and 1930's the numbers were about 100, perhaps 60 in the mornings and 35 to 40 in the afternoons, on three days a week in the Michaelmas and Lent terms. In the Easter term the numbers fell off a little. This class occupied most of his time and efforts during the most important part of his life. It is primarily on his teaching of experimental physics that his fame rests. The majority of his numerous papers are descriptions of teaching experiments, and his books are textbooks based on

manuscripts used in his practical class, but it is certain that he would not have been so good a teacher but for his early researches in electromagnetism, to which, as has been said, he returned quite late in life. Without this, the physics of the practical class might have been presented too much as an end in itself. As it was, in spite of his interest in detail, one always felt that he was thinking of physics as an ideal entity of which the laboratory was only the outward and visible sign. It was this that gave his teaching its inspiration. He showed a combination of the practical and the ideal which seems odd to the normal modern man, and perhaps unusually so to the normal physicist, but which is, one is told, characteristic of the mystic.

His experiments aimed at illustrating principles. They did this, but they also showed a good deal of ingenuity in design. It may be claimed that an experiment to teach a principle in theoretical physics should do so in the most obvious way possible. From this point of view some of Searle's experiments could be criticized as too elaborate, but it is also fair to claim that a course in practical physics should prepare men to design their own apparatus, and for this purpose examples of successful ingenuity are valuable. Certainly Searle's apparatus and methods have been adopted very widely and have influenced the teaching of physics all over the world.

He was probably a better teacher for the relative beginners of Part I, than he would have been in Part II. His lectures for Part II, though very thorough and competent, were not particularly exciting, and he never seemed quite so much at home in the lecture theatre as in the practical laboratory. To the average freshman new to Cambridge physics 'Searle's lab.' came as an exciting opening up of new ideas and methods. Perhaps in the later years this may have become less true, but mostly because the school from which the freshman had come had by then adopted a number of Searle's ideas. It was to manufacture apparatus for schools following Searle's design that Pye, till then chief mechanic at the Cavendish, set up the firm of W. G. Pye & Co., which afterwards formed the well-known radio and television firm as an off-shoot.

In the Cavendish he was a figure that generations had known with a curious mixture of awe, amusement and affection. He boasted that he had taught both father and mother of the present holder of the Cavendish Chair, and he claimed to have had more Nobel prize-winners as his pupils than any other man.

He was brusque in his manner with students, and was reputed to have reduced to tears a fair proportion of those from Girton and Newnham. He had a characteristic dry wit, as when he remarks in some hints on practical work 'In some cases the student adds the letters C.G.S. in much the same way as grocers add "esq." to customer's names.' He had a power of illustrating truth in pithy sayings—'You can speak of a clockwise rotation because you can't see through the back of the clock', or the shorter and better known 'every length has two ends.'

He was severe on those who failed to see the implications of what they said or wrote, and quoted the story about the lady who attended a course of

lectures by Sir Robert Ball, and after displaying some knowledge said 'You know, Sir Robert, I can easily understand how you discover how far away the stars are, how fast they move, and their composition; what I have never been able to understand is how you discovered their names.'

Searle was an entertaining guest to take in to dinner in a College Hall. In addition to his staccato remarks and sometimes slightly embarrassing questions, he usually had some little parlour trick to show. One of these was to hold a penny horizontally between thumb and forefinger, and by moving the forefinger allow the penny to drop with constant angular velocity. By catching the penny in the other hand, he was able to show that it reversed its 'polarity' at distances in the ratio 1 : 4 : 9. He would then show a newspaper cutting from his wallet with the headline: 'Scientist shows how to win the toss.'

In addition to stimulating students he produced a marked and lasting impression on those who were fortunate enough to be his demonstrators for any length of time. He was a very hard task master and expected a very great deal from them, but nothing was too much trouble for him if he felt that a young man was anxious to learn.

Searle was for many years a lay-reader in the Church of England, visiting churches in the villages near Cambridge, and his religion was of great importance in his life. He believed strongly in the influence of mind on bodily health, and of religion on mind. He believed that he himself had derived benefit from spiritual healing, and had given it to others. This belief did not prevent him from doing his best to help the sick in more material ways, and he did many deeds of kindness to ill people of which one only learnt by accident. As a very young man he was struck with a desire to teach the history of the church, and used a small sum of money he had received to buy a magic-lantern with which he went about the Cambridge district giving lectures on this subject.

He married in 1904 Alice Mary, née Edwards, widow of Thomas Parsons of Wimbledon. There are no children. When convalescing in 1911 from an illness which he suffered in 1910 he and his wife visited the West Indies and Canada.

Searle was a strong opponent of vivisection and experiments on animals, and made his views known in University discussions.

A fortnight before his death on 16 December 1954, he attended a tea-party given in honour of his 90th birthday at the Cavendish, and spoke very movingly of his beliefs and experiences, both in physics and religion.

GEORGE THOMSON

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A. D. King

ALAN MATHISON TURING

1912-1954

THE sudden death of ALAN TURING on 7 June 1954 deprived mathematics and science of a great original mind at the height of its power. After some years of scientific indecision, since the end of the war, Turing had found, in his chemical theory of growth and form, a theme that gave the fullest scope for his rare combination of abilities, as a mathematical analyst with a flair for machine computing, and a natural philosopher full of bold original ideas. The preliminary report of 1952, and the account that will appear posthumously, describe only his first rough sketch of this theory, and the unfulfilled design must remain a painful reminder of the loss that his early death has caused to science.

Alan Mathison Turing was born in London on 23 June 1912, the son of Julius Mathison Turing, of the Indian Civil Service, and of Ethel Sara Turing (*née* Stoney). The name 'Turing' is of Scottish, perhaps ultimately of Norman origin, the final *g* being an addition made by Sir William Turing, of Aberdeenshire, in the reign of James VI and I. The Stoneys, an English-Irish family of Yorkshire origin, produced some distinguished physicists and engineers in the nineteenth century, three of whom became Fellows of the Society; and Edith A. Stoney was one of the early women equal-to-wranglers at Cambridge (bracketed with 17th Wrangler, 1893).

Alan Turing's interest in science began early and never wavered. Both at his preparatory schools and later at Sherborne, which he entered in 1926, the contrast between his absorbed interest in science and mathematics, and his indifference to Latin and 'English subjects' perplexed and distressed his teachers, bent on giving him a well-balanced education. Many of the characteristics that were strongly marked in his later life can already be clearly seen in remembered incidents of this time: his particular delight in problems, large or small, that enabled him to combine theory with the kind of experiments he could carry out with his own hands, with the help of whatever apparatus was at hand; his strong preference for working everything out from first principles instead of borrowing from others—a habit which gave freshness and independence to his work, but also undoubtedly slowed him down, and later on made him a difficult author to read. At school home-made experiments in his study did not fit well into the routine of the house: a letter from his housemaster mentions 'Heaven knows what witches' brew blazing on a naked wooden window sill'. But before he left school his abilities,

and his obvious seriousness of purpose, had won him respect and affection, and even tolerance for his own peculiar methods.

In 1931 he entered King's College, Cambridge, as a mathematical scholar. A second class in Part I of the Tripos showed him still determined not to spend time on subjects that did not interest him. In Part II he was a Wrangler, with 'b*', and he won a Smith's Prize in 1936. He was elected a Fellow of King's in 1935, for a dissertation on the Central Limit Theorem of probability (which he discovered anew, in ignorance of recent previous work).

It was in 1935 that he first began to work in mathematical logic, and almost immediately started on the investigation that was to lead to his best known results, on computable numbers and the 'Turing machine'. The paper attracted attention as soon as it appeared and the resulting correspondence led to his spending the next two years (1936-8) in Princeton, working with Professor Alonzo Church, the second of them as Procter Fellow.

In 1938 Turing returned to Cambridge; in 1939 the war broke out. For the next six years he was fully occupied with his duties for the Foreign Office. These years were happy enough, perhaps the happiest of his life, with full scope for his inventiveness, a mild routine to shape the day, and a congenial set of fellow-workers. But the loss to his scientific work of the years between the ages of 27 and 33 was a cruel one. Three remarkable papers written just before the war, on three diverse mathematical subjects, show the quality of the work that might have been produced if he had settled down to work on some big problem at that critical time. For his work for the Foreign Office he was awarded the O.B.E.

At the end of the war many circumstances combined to turn his attention to the new automatic computing machines. They were in principle realizations of the 'universal machine' which he had described in the 1937 paper for the purpose of a logical argument, though their designers did not yet know of Turing's work. Besides this theoretical link, there was a strong attraction in the many-sided nature of the work, ranging from electric circuit design to the entirely new field of organizing mathematical problems for a machine. He decided to decline an offer of a Cambridge University Lectureship, and join the group that was being formed at the National Physical Laboratory for the design, construction and use of a large automatic computing machine. In the three years (1945-8) that this association lasted he made the first plan of the ACE, the N.P.L.'s automatic computer, and did a great deal of pioneering work in the design of sub-routines.

In 1948 he was appointed to a Readership in the University of Manchester, where work was beginning on the construction of a computing machine by F. C. Williams and T. Kilburn. The expectation was that Turing would lead the mathematical side of the work, and for a few years he continued to work, first on the design of the sub-routines out of which the larger programmes for such a machine are built, and then, as this kind of work became standardized, on more general problems of numerical analysis. From 1950 onward he

turned back for a while to mathematics and finally to his biological theory. But he remained in close contact with the Computing Machine Laboratory, whose members found him ready to tackle the mathematical problems that arose in their work, and what is more, to find the answers, by that combination of powerful mathematical analysis and intuitive short cuts that showed him at heart more of an applied than a pure mathematician.

He was elected to the Fellowship of the Society in 1951.

For recreation he turned mostly to those 'home-made' projects and experiments, self-contained both in theory and practice, that have already been mentioned: they remained a ruling passion up to the last hours of his life. The rule of the game was that everything was to be done with the materials at hand, and worked out from data to be found in the house, or in his own head. This sort of self-sufficiency stood him in good stead in starting on his theory of 'morphogenesis', where the preliminary reading would have drowned out a more orthodox approach. In everyday life it led to a certain fondness for the gimcrack, for example the famous Bletchley bicycle, the chain of which would stay on if the rider counted his pedal-strokes and executed a certain manoeuvre after every seventeen strokes.

After the war, feeling in need of violent exercise, he took to long distance running, and found that he was very successful at it. He won the 3 miles and 10 miles championships of his club (the Walton Athletic Club), both in record time, and was placed fifth in the A.A.A. Marathon race in 1947. He thought it quite natural to put this accomplishment to practical use from time to time, for example by running some nine miles from Teddington to a technical conference at the Post Office Research Station in North London, when the public transport proved tedious.

In conversation he had a gift for comical but brilliantly apt analogies, which found its full scope in the discussions on 'brains *v.* machines' of the late 1940's. He delighted in confounding those who, as he thought, too easily assumed that the two things are separated by an impassable gulf, by challenging them to produce an examination paper that could be passed by a man, but not by a machine. The unexpected element in human behaviour he proposed, half seriously, to imitate by a random element, or roulette-wheel, in the machine. This, he said, would enable proud owners to say 'My machine' (instead of 'My little boy') 'said such a funny thing this morning'.

Those who knew Turing will remember most vividly the enthusiasm and excitement with which he would pursue any idea that caught his interest, from a conversational hare to a difficult scientific problem. Nor was it only the pleasure of the chase that inspired him. He would take the greatest pains over services, large or small, to his friends. His colleagues in the computing machine laboratory found him still as ready as ever with his help for their problems when his own interests were fully engaged with his bio-chemical theory; and, as another instance, he gave an immense amount of thought and care to the selection of the presents which he gave to his friends and their children at Christmas.

His death, at a time when he was fully absorbed in his scientific work, was a great and sad loss to his friends, as well as to the wider world of science.

Scientific work

The varied titles of Turing's published work disguise its unity of purpose. The central problem with which he started, and to which he constantly returned, is the extent and the limitations of mechanistic explanations of nature. All his work, except for three papers in pure mathematics (1935*b*, 1938*a* and *b*) grew naturally out of the technical problems encountered in these inquiries. His way of tackling the problem was not by philosophical discussion of general principles, but by mathematical proof of certain limited results: in the first instance the impossibility of the too sanguine programme for the complete mechanization of mathematics, and in his final work, the possibility of, at any rate, a partial explanation of the phenomena of organic growth by the 'blind' operation of chemical laws.

1. Mathematical logic

The Hilbert decision-programme of the 1920's and 30's had for its objective the discovery of a general process, applicable to any mathematical theorem expressed in fully symbolical form, for deciding the truth or falsehood of the theorem. A first blow was dealt at the prospects of finding this new philosopher's stone by Gödel's incompleteness theorem (1931), which made it clear that truth or falsehood of A could not be equated to provability of A or not- A in any finitely based logic, chosen once for all; but there still remained in principle the possibility of finding a mechanical process for deciding whether A , or not- A , or neither, was formally provable in a given system. Many were convinced that no such process was possible, but Turing set out to demonstrate the impossibility rigorously. The first step was evidently to give a definition of 'decision process' sufficiently exact to form the basis of a mathematical proof of impossibility. To the question 'What is a "mechanical" process?' Turing returned the characteristic answer 'Something that can be done by a machine', and he embarked on the highly congenial task of analyzing the general notion of a computing machine. It is difficult to-day to realize how bold an innovation it was to introduce talk about paper tapes and patterns punched in them, into discussions of the foundations of mathematics. It is worth while quoting from his paper (1937*a*) the paragraph in which the computing machine is first introduced, both for the sake of its content and to give the flavour of Turing's writings.

'1. Computing machines

'We have said that the computable numbers are those whose decimals are calculable by finite means. This requires rather more explicit definition. No real attempt will be made to justify the definitions given until we reach §9. For the present I shall only say that the justification lies in the fact that the human memory is necessarily limited.

'We may compare a man in the process of computing a real number to a machine which is only capable of a finite number of conditions $q_1, q_2, \dots, q_R$ which will be called " m -configurations". The machine is supplied with a "tape" (the analogue of paper) running through it, and divided into sections (called "squares") each capable of bearing a "symbol". At any moment there is just one square, say the r -th, bearing the symbol $\mathfrak{S}(r)$ which is "in the machine". We may call this square the "scanned square". The symbol on the scanned square may be called the "scanned symbol". The "scanned symbol" is the only one of which the machine is, so to speak, "directly aware". However, by altering its m -configuration the machine can effectively remember some of the symbols which it has "seen" (scanned) previously. The possible behaviour of the machine at any moment is determined by the m -configuration q_n and the scanned symbol $\mathfrak{S}(r)$. This pair $q_n, \mathfrak{S}(r)$ will be called the "configuration": thus the configuration determines the possible behaviour of the machine. In some of the configurations in which the scanned square is blank (*i.e.* bears no symbol) the machine writes down a new symbol on the scanned square: in other configurations it erases the scanned symbol. The machine may also change the square which is being scanned, but only by shifting it one place to right or left. In addition to any of these operations the m -configuration may be changed. Some of the symbols written down will form the sequence of figures which is the decimal of the real number which is being computed. The others are just rough notes to "assist the memory". It will only be these rough notes which will be liable to erasure.

'It is my contention that these operations include all those which are used in the computation of a number. The defence of this contention will be easier when the theory of the machines is familiar to the reader.'

In succeeding paragraphs he gave arguments for believing that a machine of this kind could be made to do any piece of work which could be done by a human computer obeying explicit instructions given to him before the work starts. A machine of the kind he had described could be made for computing the successive digits of π , another for computing the successive prime numbers, and so forth. Such a machine is completely specified by a table, which states how it moves from each of the finite sets of possible 'configurations' to another. In the computations mentioned above, of π and of the successive primes, the machine may be supposed to be designed for its special purpose. It is supplied with a blank tape and started off. But we may also imagine a machine supplied with a tape already bearing a pattern which will influence its subsequent behaviour, and this pattern might be the table, suitably encoded, of a particular computing machine, X . It could be arranged that this tape would cause the machine, M , into which it was inserted to behave like machine X . Turing proved the fundamental result that there is a 'universal' machine, U (of which he gave the table), which can be made to do the

work of any assigned special-purpose machine, that is to say to carry out any piece of computing, if a tape bearing suitable 'instructions' is inserted into it. The machine U is so constructed that, presented with a tape bearing any arbitrary pattern it will move through a determinate, in general endless, succession of configurations; and it may or may not print at least one digit, 0 or 1. If it does, the pattern is 'circle-free'. It is therefore a problem, for which a decision process might be sought, to determine from inspection of a tape, whether or not it is circle-free. By means of a Cantor diagonal argument, Turing showed that no instruction-tape will cause the machine U to solve this problem, i.e. no pattern P is such that U , when presented with P followed by an arbitrary pattern $\mathcal{Y}$, will print 0 if $\mathcal{Y}$ is 'circle-free', and 1 if it is not. If Turing's thesis is accepted, that the existence of a *method* for solving such a problem means the existence of a machine (or an instruction-tape for the universal machine U) that will solve it, it follows that the discovery of a process for discriminating between circle-free and other tapes is an insoluble problem, in an absolute and inescapable sense. From this basic insoluble problem it was not difficult to infer that the Hilbert programme of finding a decision method for the axiomatic system, $\mathcal{Z}$, of elementary number-theory, is also impossible.

In the application he had principally in mind, namely, the breaking down of the Hilbert programme, Turing was unluckily anticipated by a few months by Church, who proved the same result by means of his ' λ -calculus'. An offprint arrived in Cambridge just as Turing was ready to send off his manuscript. But it was soon realized that Turing's 'machine' had a significance going far beyond this particular application. It was shown by Turing (1937*b*) and others, that the definitions of 'general recursive' (by Gödel in 1931 and Kleene in 1935), ' λ -definable' (by Church in 1936) and 'computable' (Turing 1937*a*) have exactly the same scope, a fact which greatly strengthened the belief that they describe a fundamentally important body of functions. Turing's treatment has the merit of making a particularly convincing case for the acceptance of these and no other processes, as genuinely constructive; and it turned out to be well adapted for use in finding other insoluble problems, e.g. in the theory of groups and semi-groups.

Turing's other major contribution to this part of mathematical logic, the paper (1939) on systems of logic based on ordinals, has received less attention than (1937*a*), perhaps owing to its difficulty. The method of Gödel for constructing an undecidable sentence in any finitely based logic, L , i.e. a sentence expressible, but neither provable nor disprovable, in L , has led to the consideration of infinite families of 'logics', L_α , one for each ordinal α , where $L_{\alpha+1}$ is formed from L_α by the adjunction as an axiom of a sentence undecidable for L_α , if such exist, and L_α for limit ordinals α has as 'provable formulae', the union of the sets P_β ($\beta < \alpha$), where P_β is the set of provable formulae in L_β . The process must terminate for some $\gamma < \omega_1$, since the total set of formulae (which does not change) is countable. This procedure opens up the possibility of finding a logic that is complete, without violating Gödel's principle, since

L_α may not be finitely based if α is a limit ordinal. Rosser investigated this possibility in 1937, using the 'classical' non-constructive theory of ordinals. Turing took up the proposal, but with the proviso that, although some non-constructive steps must be made if a complete logic is to be attained, a strict watch should be kept on them. He first introduced a new theory of constructive ordinals, or rather of formulae (of Church's λ -calculus) representing ordinals; and he showed that the problem of deciding whether a formula represents an ordinal (in a plausible sense) is insoluble, in the sense of his earlier paper. A formula $\mathbf{L}$ of the λ -calculus is a *logic* if it gives a means of establishing the truth of number-theoretic theorems; formally, if $\mathbf{L}(\mathbf{A})$ conv. **2** implies that $\mathbf{A}(\mathbf{n})$ conv. **2** for each $\mathbf{n}$ representing a natural number. The *extent* of $\mathbf{L}$ is the set of $\mathbf{A}$'s such that $\mathbf{L}(\mathbf{A})$ conv. **2**, i.e. roughly speaking, the set of $\mathbf{A}$'s for which $\mathbf{L}$ proves $\mathbf{A}(\mathbf{n})$ true for all $\mathbf{n}$. An *ordinal logic* is now defined to be a formula $\mathbf{\Lambda}$, such that $\mathbf{\Lambda}(\mathbf{\Omega})$ is a logic whenever $\mathbf{\Omega}$ represents an ordinal; and $\mathbf{\Lambda}$ is *complete* if every number-theoretic theorem that is true, is probable in $\mathbf{\Lambda}(\mathbf{\Omega})$ for some $\mathbf{\Omega}$, i.e. if given $\mathbf{A}$ such that $\mathbf{A}(\mathbf{n})$ conv. **2** for each $\mathbf{n}$ representing a natural number, $\mathbf{\Lambda}(\mathbf{\Omega}, \mathbf{A})$ conv. **2** for some $\mathbf{\Omega}$ (depending on $\mathbf{A}$). It is next shown, by an example, that formulae $\mathbf{\Omega}_1, \mathbf{\Omega}_2$ may represent the same ordinal, but yet make $\mathbf{\Lambda}(\mathbf{\Omega}_1)$ and $\mathbf{\Lambda}(\mathbf{\Omega}_2)$ different logics, in the sense that they have different extents. An ordinal logic for which this cannot happen is *invariant*. It is only in invariant logics that the 'depth' of a theorem can be measured by the size of the ordinal required for its proof. The main theorems of the paper state (1) that complete ordinal logics and invariant ordinal logics exist, (2) that no complete and invariant ordinal logic exists.

This paper is full of interesting suggestions and ideas. In §4 Turing considers, as a system with the minimal departure from constructiveness, one in which number-theoretic problems of some class are arbitrarily assumed to be soluble: as he puts it, 'Let us suppose that we are supplied with some unspecified means of solving number-theoretic problems; a kind of oracle, as it were.' The availability of the oracle is the 'infinite' ingredient necessary to escape the Gödel principle. It also obviously resembles the stages in the construction of a proof by a mathematician where he 'has an idea', as distinct from making mechanical use of a method. The discussion of this and related matters in §11 ('The purpose of ordinal logics') throws much light on Turing's views on the place of intuition in mathematical proof. In the final rather difficult §12 the idea adumbrated by Hilbert in 1922 of recursive definitions of order-types other than ω received its first detailed exposition.

Besides these two pioneering works, and the papers (1937*b*, *c*), arising directly out of them, Turing published four papers of predominantly logical interest. (A) The paper (1942*a*), with M. H. A. Newman, on a formal question in Church's theory of types. (B) A 'practical form of type-theory' (1948*b*) is intended to give Russell's theory of types a form that could be used in ordinary mathematics. Since the more flexible Zermelo-von Neumann set-theory has been generally preferred to type-theory by mathematicians, this paper has received little attention. It contains a number of interesting ideas,

in particular a definition of 'equivalence' between logical systems (p. 89). (C) The use of dots as brackets (1942*b*), an elaborate discussion of punctuation in symbolic logic. Finally, (D) contains the proof (1950*a*) of the insolubility of the word-problem for semi-groups with cancellation. A finitely generated semi-group *without* cancellation is determined by choosing a finite set of pairs of words, (A_i, B_i) ($i=1, \dots k$) of some alphabet, and declaring two words to be 'equivalent' if they can be proved so by the use of the equations $PA_iQ = PB_iQ$, where P and Q can be arbitrary words (possibly empty). The word-problem for such a semi-group is to find a process which will decide whether or not two given words are equivalent. The insolubility of this problem can be brought into relation with the fundamental insoluble machine-tape problem. The table of a computing machine states, for each configuration, what is the configuration that follows it. Since a configuration can be denoted by a 'word', in letters representing the internal configurations and tape-symbols, this table gives a set of pairs of words which, when suitably modified, determine a semi-group with insoluble word problem. So much was proved by E. L. Post in 1947. The question becomes much more difficult if the semi-group is required to satisfy the cancellation laws, ' $AC = BC$ implies $A = B$ ' and ' $CA = CB$ implies $A = B$ ', since now a condition is imposed on account of its mathematical interest, and not because it arises naturally from the machine interpretation. This was the step taken by Turing in (1950*a*). (For a helpful discussion and analysis of this difficult paper see the long review by W. W. Boone, *J. Symbolic Logic*, **17** (1952) 74.)

2. Three mathematical papers

Shortly before the war Turing made his only contributions to mathematics proper.

The paper 1938*a* contains an interesting theorem on the approximation of Lie groups by finite groups: if a (connected) Lie group, L , can for arbitrary $\varepsilon > 0$ be ε -approximated by a finite group whose multiplication law is an ε -approximation to that of L , in the sense that the two products of any two elements are within ε of each other, then L must be both compact and abelian. The theory of representations of topological groups is used to apply Jordan's theorem on the abelian invariant subgroups of finite groups of linear transformations.

Paper (1938*b*) lies in the domain of classical group theory. Results of R. Baer on the extensions of a group are re-proved by a more unified and simpler method.

Paper (1943)—submitted in 1939, but delayed four years by war-time difficulties—shows that Turing's interest in practical computing goes back at least to this time. A method is given for the calculation of the Riemann zeta-function, suitable for values of t in a range not well covered by the previous work of Siegel and Titchmarsh. The paper is a straightforward but highly skilled piece of classical analysis. (The post-war paper (1953*a*) describes an

attempt to apply a modified form of this process, which failed owing to machine trouble.)

3. *Computing machines*

Apart from the practical 'programmer's handbook', only two published papers (1948*a* and 1950*b*) resulted from Turing's work on machines. When binary fractions of fixed length are used (as they must be on a computing machine) for calculations involving a very large number of multiplications and divisions, the necessary rounding-off of exact products introduces cumulative errors, which gradually consume the trustworthy digits as the computation proceeds. The paper (1948*a*) investigates questions of the following type: how many figures of the answer are trustworthy if k figures are retained in solving n linear equations in n unknowns? The answer depends on the method of solution, and a number of different ones are considered. In particular it is shown that the ordinary method of successive elimination of the variables does not lead to the very large errors that had been predicted, save in exceptional cases which can be specified.

The other paper (1950*b*) arising out of his interest in computing machines is of a very different nature. This paper, on computing machines and intelligence, contains Turing's views on some questions about which he had thought a great deal. Here he elaborates his notion of an 'examination' to test machines against men, and he examines systematically a series of arguments commonly put forward against the view that machines might be said to think. Since the paper is easily accessible and highly readable, it would be pointless to summarize it. The conversational style allows the natural clarity of Turing's thought to prevail, and the paper is a masterpiece of clear and vivid exposition.

The proposals (1953*b*) for making a computing machine play chess are amusing, and did in fact produce a defence lasting 30 moves when the method was tried against a weak player; but it is possible that Turing underestimated the gap that separates combinatory from position play.

4. *Chemical theory of morphogenesis*

For the following account of Turing's final work I am indebted to Dr N. E. Hoskin, who with Dr B. Richards is preparing an edition of the material for publication.

The work falls into two parts. In the first part, published (1952) in his lifetime, he set out to show that the phenomena of morphogenesis (growth and form of living things) could be explained by consideration of a system of chemical substances whose concentrations varied only by means of chemical reactions, and by diffusion through the containing medium. If these substances are considered as form-producers (or 'morphogens' as Turing called them) they may be adequate to determine the formation and growth of an organism, if they result in localized accumulations of form-producing substances. According to Turing the laws of physical chemistry are sufficient to

account for many of the facts of morphogenesis (a view similar to that expressed by D'Arcy Thompson in *Growth and form*).

Turing arrived at differential equations of the form

$$\frac{\partial X_i}{\partial t} = f_i(X_1, \dots, X_n) + \mu \nabla^2 X_i, \quad (i = 1, \dots, n)$$

for n different morphogens in continuous tissue; where f_i is the reaction function giving the rate of growth of X_i , and $\nabla^2 X_i$ is the rate of diffusion of X_i . He also considered the corresponding equations for a set of discrete cells. The function f_i involves the concentrations, and in his 1952 paper Turing considered the X_i 's as variations from a homogeneous equilibrium. If, then, there are only small departures from equilibrium, it is permissible to linearize the f_i 's, and so linearize the differential equations. In this way he was able to arrive at the conditions governing the onset of instability. Assuming initially a state of homogeneous equilibrium disturbed by random disturbances at $t = 0$, he discussed the various forms instability could take, on a continuous ring of tissue. Of the forms discussed the most important was that which eventually reached a pattern of stationary waves. The botanical situation corresponding to this would be an accumulation of the relevant morphogen in several evenly distributed regions around the ring, and would result in the main growth taking place at these points. (The examples cited are the tentacles of *Hydra*, and whorled leaves.) He also tested the theory by obtaining numerical solutions of the equations, using the electronic computer at Manchester. In the numerical example, in which two morphogens were supposed to be present in a ring of twenty cells, he found that a three or four lobed pattern would result. In other examples he found two-dimensional patterns, suggestive of dapppling; and a system on a sphere gave results indicative of gastrulation. He also suggested that stationary waves in two dimensions could account for the phenomena of phyllotaxis.

In his later work (as yet unpublished) he considered quadratic terms in the reaction functions in order to take account of larger departures from the state of homogeneous equilibrium. He was attempting to solve the equations in two dimensions on the computer at the time of his death. The work is in existence, but unfortunately is in a form that makes it extremely difficult to discover the results he obtained. However, B. Richards, using the same equations, investigated the problem in the case where the organism forms a spherical shell and also obtained numerical results on the computer. These were compared with the structure of *Radiolaria*, which have spikes on a basic spherical shell, and the agreement was strikingly good. The rest of this part of Turing's work is incomplete, and little else can be obtained from it. However, from Richards's results it seems that consideration of quadratic terms is sufficient to determine practical solutions, whereas linear terms are really only sufficient to discuss the onset of instability.

The second part of the work is a mathematical discussion of the geometry of phyllotaxis (i.e. of mature botanical structures). Turing discussed many

ways of classifying phyllotaxis patterns and suggested various parameters by which a phyllotactic lattice may be described. In particular, he showed that if a phyllotactic system is Fibonacci in character, it will change, if at all, to a system which has also Fibonacci character. This is in accordance with observation. However, most of this section was intended merely as a description preparatory to his morphogenetic theory, to account for the facts of phyllotaxis; and it is clear that Turing did not intend it to stand alone.

The wide range of Turing's work and interests have made the writer of this notice more than ordinarily dependent on the help of others. Among many who have given valuable information I wish to thank particularly Mr R. Gandy, Mr J. H. Wilkinson, Dr B. Richards and Dr N. E. Hoskin; and Mrs Turing, Alan Turing's mother, for constant help with biographical material.

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 [A second paper on morphogenesis is being prepared for publication by N. E. Hoskin and B. Richards, based on work left by Turing.]

* Received four years earlier (7 March 1939).



